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GEOLOGY AND CLIMATOLOGY FROM THE OCEAN ABYSS[1]

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ONE property alone of the ocean—its immensity—determines that the sediments accumulating in its great central basin shall be different from those accumulating elsewhere. Most of the ocean abyss, that vast basin beyond the outer margins of the continental shelves, is too far from the land to receive more than an insignificant fraction of the finest mineral particles washed from the land by rivers and waves. Streams fill small ponds and reservoirs with mud in a comparatively few years; and rivers, by adding to their deltas, diminish the size of great inland seas such as the Mediterranean, by measurable amounts. In describing the changes in the delta of the Rhone, Sir Charles Lyell wrote, "Psalmodi was an island in 815, and is now two leagues from the sea. Several old lines of towers and seamarks occur at different distances from the present coast, all indicating the successive retreat of the sea, for each line has in its turn become useless to mariners; which may well be conceived, when we state that the Tower of Tignaux, erected on the shore so late as the year 1737, is already a mile remote from it." The great rivers of the world have poured many cubic miles of mud into the ocean and built huge deltas, but even the greatest of these rivers is powerless to project its muddy stream so far into the abyssal realm of the ocean as to make a significant contribution to the deposits accumulating there.

Only that part of the land waste taken into solution by running water reaches and eventually makes a permanent contribution to the deposits on the floor of the ocean abyss. Of the substances so transported calcium is dominant as a maker of oceanic sediments and silica is next. By far the greater bulk of the deposits covering the abyssal parts of the North Atlantic consist of calcium carbonate and silica shells whose substance was abstracted from the sea water by small organisms. However, when we consider that much of the calcium and silica brought each year to the sea are deposited on the continental shelves and in coastal waters and that only a small fraction of the annual supply reaches the deep ocean basin, we begin to realize how slow oceanic deposition must be. Studies by H. Lohman and by W. Schott, both of Germany, and by the writer and other members of the Geological Survey, U. S. Department of the Interior, agree in indicating that the carpet of sediment covering the floor of the abyssal part of the ocean increases each year by the addition of a mere film of mud between one hundredth and one thousandth of an inch thick. Because of this slow rate of accumulation more earth history is compressed into a short column of such sedi-

[1] Published with the permission of the Director, Geological Survey.

ment than can be found in any shallow water or terrestrial sedimentary record. In consequence of that fact geologists have long desired access to the record written in abyssal sediments.

I

Cores as much as 10 feet long of undisturbed sediment can now be successfully taken from the ocean bottom by means of an ingenious coring device designed by Dr. C. S. Piggot of the Carnegie Geophysical Laboratory in Washington. This device shoots the core barrel into the mud by means of a gun loaded with coarse pellet cannon powder. This apparatus and its operation were described by Piggot in the March 1938 issue of the SCIENTIFIC MONTHLY. Trials along the Atlantic coast in 1935, in water ranging from 1,200 to 7,500 feet in depth, showed that the apparatus was ready for deep-sea work. Then, in May 1936, the Western Union Telegraph Company's cable ship *Lord Kelvin* stood out of Halifax bound for Falmouth, England, to repair, en route, a trans-Atlantic cable. She carried as guest Dr. Piggot, who had with him the coring device. On that trip he took 11 cores at the stations shown in Fig. 1. These cores ranged in length from 10 feet to 1 foot 1 inch and averaged 7 feet 8½ inches. The very short one was short because the core barrel struck rock. At the places where these cores were taken the water ranges in depth from 4,200 feet (on the top of the mid-Atlantic ridge) to 15,840 feet. Back in the laboratory the core barrels were carefully cut open lengthwise without disturbing the sediment, so that these compact records of the past could be examined and studied with microscopes and analyzed piece by piece from as many points of view as could be devised.[2]

In our interpretation of the deposits penetrated by these cores we have followed the principle long ago laid down by Lyell, that the present is the key to the past. The sediments now accumulating on the ocean floor are therefore the standard, or norm, with which all the older sediments below must be compared. They contain the remains of the same kinds of organisms that are now living in the surface waters above and that live on the bottom mud itself. They are, in short, the resultant of all the factors of the deep oceanic environment as we know them to-day.

Predominantly the modern sediments consist of the skeletal parts of minute free-floating or plankton organisms that

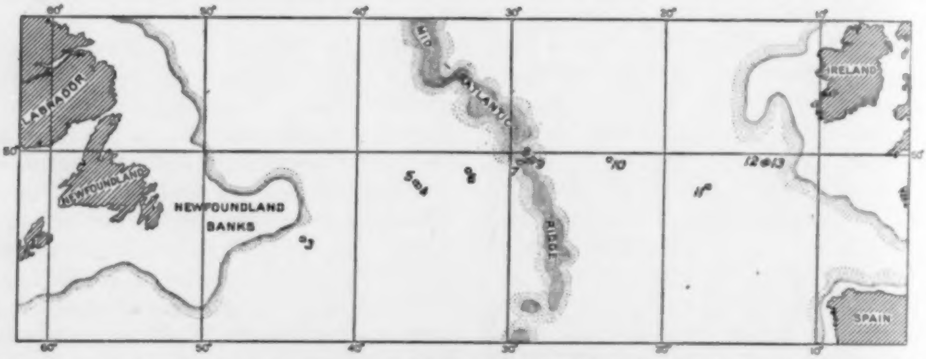


FIG. 1. CHART OF A PART OF THE NORTH ATLANTIC OCEAN SHOWING THE LOCATION OF THE DEEP-SEA CORES

[2] The cores were studied by M. N. Bramlette, Jos. A. Cushman, L. G. Henbest, K. E. Lohman, P. D. Trask and the writer, all of the United States Geological Survey. In addition to these geologists, Dr. W. L. Tressler, of the University of Buffalo, studied the Ostracoda, and Dr. H. A. Rehder, of the U. S. National Museum, studied the Mollusca (chiefly pteropods).

dwell in the sun-lit surface water. Most of these organisms live but a comparatively short time and then settle into the dark abyss, producing, as it were, a gentle rain of minute limy and siliceous skeletons. Thus it is that some of the dissolved substances from the land are eventually transformed by minute organisms into durable particles of lime and silica and find their way to the floor of the ocean.

To be sure, not the same kind of sediment is accumulating on all parts of the ocean floor to-day. That forming in polar regions differs markedly from that forming in temperate latitudes. A comparable degree of difference is also to be found between sediments formed in water less than approximately 16,000 feet deep and in water of greater depth. In the consideration of these North Atlantic cores, however, we shall be concerned chiefly with only two types of oceanic sediment that are now forming—foraminiferal ooze and blue mud.

At all the core stations except one, the ocean floor is carpeted with foraminiferal ooze. The westernmost core was taken in the blue mud zone. Because departures from these two types of sediment are the means by which we recognize changed conditions in the world of the past, it is necessary to have in mind the distinguishing features of these types. As the name implies, foraminiferal ooze is characterized by an abundance of the minute limy shells of Foraminifera—a class of unicellular animals. Most of these are the globose shells of Globigerina, Globorotalia and other similar surface-dwelling or pelagic genera. Intermingled with the pelagic forms is a lesser number of bottom-dwelling forms whose shells are more varied in both form and texture. Fragments of foraminiferal shells, ranging from nearly complete individuals down to finely comminuted particles, account for a considerable bulk of the sediment. Mixed with the comminuted shells are still finer limy particles of unknown origin and also great numbers of minute limy plates secreted by unicellular plants that live in the surface waters. These microscopic plants are brown algae belonging to the Coccolithophoridae. The commonest ones have a globular coating or shell made up of calcium carbonate plates of distinctive shapes. When the plant dies the plates, which are known as coccoliths, readily separate so that in the sediment only the individual plates are found. Most of these plates are less than one ten thousandth of an inch across, yet they are so numerous that locally they make up as much as 10 per cent. of the sediment. Scattered through the limy constituents of the mud are the

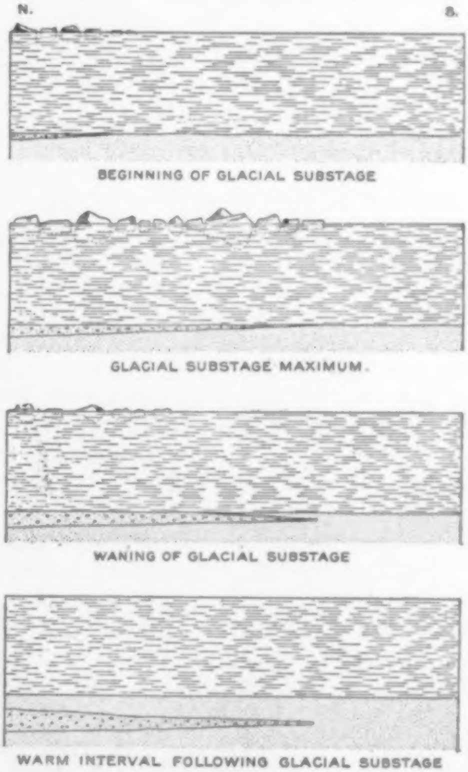


FIG. 2. DIAGRAM OF GLACIAL MARINE SEDIMENT

ILLUSTRATING THE GROWTH OF A WEDGE. COARSE STIPPLING REPRESENTS GLACIAL MARINE SEDIMENT; FINE STIPPLING REPRESENTS FORAMINIFERAL OOZE.

ornate silica skeletons of diatoms and Radiolaria. Minute clay particles from the land, motes of atmospheric dust, and, less consistently, particles of volcanic ash make up the remainder.

The blue mud that is forming near the foot of the continental shelf off the Newfoundland Banks, where the westernmost core of the series was taken, is characterized by an abundance of clay and fine silt —constituents derived from the land and continually swept off the continental shelf. This terrigenous constituent in effect dilutes the normal oceanic sediment of organic remains so that foraminiferal shells, diatom tests and the like, are less numerous per unit volume than they are in the foraminiferal ooze. Blue mud zones, or belts, skirt the continents and define the limits to which considerable amounts of land waste are transported in suspension. Seaward, blue mud grades gradually into foraminiferal ooze.

This then is the present-day areal distribution of these two dominant kinds of sediment in the North Atlantic; but it was not always so, for at various times in the past not only the distribution but also the kinds of deposits were different, for in these cores we have a succession of layers one above the other.

The layers differ somewhat, both in number and thickness, from one core to another but less than might be expected in cores taken so many miles apart. Nevertheless, because of this variation it will be simpler to consider here the layers in a single core whose sedimentary record is both long and clearly written. The core from station 7, just west of the mid-Atlantic ridge in 10,680 feet of water, best fulfills this requirement. Furthermore, this core is so nearly like most of the others that comparisons with them can easily be made.

At the top of the core from station 7 is foraminiferal ooze a trifle more than one foot thick. To the unaided eye it appears to be nearly homogeneous, but the microscope shows a significant departure from homogeneity. Tiny particles of volcanic ash, which are scarce at the top of the core, become progressively more numerous until at a place about 9 inches down there are one or two thin layers in which they are plentiful. Below that they are absent. The word "ash" suggests that the particles are cinder-like but instead, like most volcanic ash, they are flakes or shards of clear, gray glass—usually parts of glass bubbles that formed, and broke into bits, as the molten rock frothed up out of a volcano in an explosive eruption. Such glass shards are so small and thin that they are blown far above the volcano and thence carried great distances by the wind.

The important thing, however, is that this same distribution of volcanic ash was found in all the cores except two—the very short one that struck rock and the westernmost one in the blue mud zone. This means that at one definite episode a volcano of the explosive type threw out, in a short series of eruptions, ash that settled as a thin layer extending across the North Atlantic Ocean. Even allowing for the very slow settling of these particles through several miles of water, most of them must have reached the bottom within a year or two after the eruptions. Thus, the base of such a layer must be very nearly contemporaneous throughout its entire extent and is, therefore, unsurpassed as a means of correlating layers of sediment from core to core.

The base of this ash layer, or zone, is not at the same position in all the cores. In some it is only a few inches from the top, in others it is near the middle; and it evidently must lie below the bottom of the westernmost core, for that core, despite its length of nearly 10 feet, has ash shards scattered sparsely all through it but no layers wherein the particles are concentrated. The layer of sediment between the base of the ash zone and the tops of the cores has, of course, all been deposited since the ash fell, and where

this layer is thick it means that there the ocean floor is being built up more rapidly than where the layer is thin. Thus, we learn that sedimentation is more than ten times as rapid in the blue mud zone off the Newfoundland Banks as it is at the site of the core just west of the mid-Atlantic ridge and that it is slowest on the crest of the mid-Atlantic ridge. The position of the ash zone in the cores also shows that, in general, sedimentation is faster in the great basin east of the mid-Atlantic ridge than it is in the basin west of the ridge.

The upward scattering of the ash shards through the sediment above the base of the ash zone where they are most plentiful, is apparently a testament to the activity of mud-feeding animals that inhabit the deeps. Animals like the sea cucumbers and sea urchins are voracious and indiscriminate feeders. They move slowly over the mud, scoop it up, and pass it through their intestinal tracts for what nutrients it contains and, in so doing, leave the mineral particles at a slightly higher level than they were before. When the volcanic ash covered the sea bottom the animals then living must have taken in mud that consisted largely of shards, but as time went on the continual rain of minute shells and other organic remains diluted the ash and so successive generations of mud feeders encountered fewer and fewer shards until now their descendants only rarely find these abrasive particles to plague them. A similar, though somewhat thinner, zone containing volcanic ash was found in the lower parts of four of the cores west of the mid-Atlantic ridge.

We found no clues to aid us in locating the volcanoes that produced the ash in either zone. Both the Azores and Iceland have volcanoes that could produce ash of that chemical composition; and volcanoes in either place, or elsewhere, might have been the source.

Aside from the fortuitous occurrence of the volcanic ash zone, the uppermost layer of foraminiferal ooze in the representative core is virtually identical with the sediment now forming in that part of the North Atlantic. This must mean that the environment has not changed materially during that interval. But lower in the core are four layers of distinctly different sediment that betoken radical changes in the oceanic environment of the past. The uppermost of these layers is approximately one foot below the top of the core and is itself approximately one foot thick. The others, which are somewhat thinner, follow in sequence below and are separated from one another by layers of foraminiferal ooze each of which is nearly a foot thick. The sediment in these four layers is characterized by an abundance of silt and sand, and a sprinkling of pebbles some of which are nearly half an inch across. Foraminifera shells are scarce, coccoliths are absent and the sediment as a whole is rather poor in lime.

The uppermost of these sandy layers apparently extends all across the North Atlantic, for it was found just a little way below the upper ash zone in all the cores except the westernmost one and of course the very short one at core station 11. Perhaps the other three sandy layers also extend across the Atlantic, but we found them only in the cores taken at stations 4 to 8, in the western part of the ocean. Of the cores taken east of the mid-Atlantic ridge only the easternmost one contains more than one sandy layer. It contains one well-defined and two lower rather ill-defined layers.

No marine currents, short of those fabulous ones imagined to exist in the western Atlantic by pre-Columbian sailors, could have brought from the land the sandy and gravelly material in these four layers, and certainly the ocean has not been repeatedly drained nor has its floor been repeatedly uplifted several miles so that brooks and rivers could have distributed it. Floating ice seems to be the only agent capable of transporting so

much coarse detritus so far from the land. We concluded, therefore, that only at a time when vast continental ice sheets spread over the land and continually pushed their edges out to sea could the ocean contain enough drift ice (particularly ice laden with coarse detritus scoured from the land) to account for the sandy and pebbly layers. Such a time could have been none other than the Pleistocene ice age. We call these deposits, therefore, glacial marine deposits.

This conclusion was beautifully confirmed by the study of the Foraminifera which furnish an index of surface water temperature. In the cold northern waters live comparatively few species, though individuals are numerous and their shells are robust. In the warmer waters of the Gulf Stream and the southern parts of the North Atlantic the Foraminifera are represented by a much greater number of species, including both thick and thin shelled forms. Although several species are common to both environments the assemblage of forms found in either environment is distinctive. It was found that the cold-water forms in the cores were restricted to certain zones that coincided exactly with the glacial marine layers as recognized by their physical characteristics. Furthermore, the Foraminifera indicated that the sediment between the uppermost pair of glacial layers had been deposited in water nearly, or quite, as cold as the glacial deposits. In this the Foraminifera again agreed with the findings based upon the physical features, for, unlike the other non-glacial deposits, the texture of this particular sediment is intermediate between glacial and non-glacial types and it contains fewer shells than the other layers of foraminiferal ooze.

We shall try now to visualize the conditions in the North Atlantic some thousands of years past when the glacial marine deposits were forming. We must conclude from the great areal extent of the glacial layers that the sea southward beyond the fiftieth parallel of latitude contained much berg ice from continental glaciers and doubtless also much shore and sea ice. During glacial epochs the sea level goes down because much of the water that falls on the land as rain or snow is converted to ice and remains locked up in the vast ice sheets that spread over the land. It has been estimated, first by Antevs and later by Daly, that during the last glacial stage the sea level was approximately 300 feet below

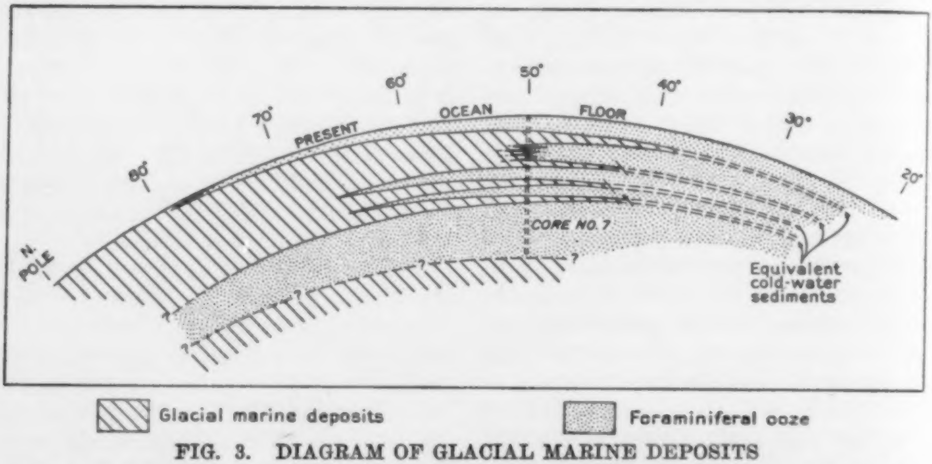


FIG. 3. DIAGRAM OF GLACIAL MARINE DEPOSITS

SHOWING THE INFERRED DISTRIBUTION ALONG A MERIDIONAL SECTION THROUGH CORE NUMBER 7. THE VERTICAL SCALE IS TREMENDOUSLY EXAGGERATED.

its present position. Probably extensive shoal-water platforms furnished large volumes of sand, gravel, and other detritus not only to the glacier ice shoved out across them but also to grounded sea ice.

Drawing a comparison with conditions as they now exist in both polar seas, we may infer that the drift ice did not form a close pack or continuous sheet over the site of the cores but instead was broken and probably melting rather actively, for the glacial marine zones contain some pelagic Foraminifera and diatoms that must have lived in open water. Such Foraminifera and diatoms are rare or absent from the bottom deposits beneath the continuous sheets of pack-ice in both the Arctic and Antarctic.

Active melting of the drift ice at essentially its southern limit in this part of the North Atlantic may well have been due to the Gulf Stream, which probably flowed there much as it does to-day but with somewhat less volume and less heat during the glacial maxima. A warm current meeting extensive areas of drift ice offers optimal conditions for the growth of pelagic organisms. The cold polar water, long stored in the dark under the ice, becomes rich in dissolved phosphates, nitrates, oxygen and probably also silica, because the few organisms that live there fail to use up these substances. With these nutrient substances available in generous amounts the indigenous plankton and also some of the pelagic organisms carried up from more temperate latitudes by the warm current thrive in great abundance. Many kinds of the warmer-water forms are killed off when they reach the polar water and these go at once to the bottom to become part of the sediment.

The convergence of a warm northward-pushing current upon the cold water of a berg-dotted sea provides a mechanism that readily accounts for a rather abrupt transition between the glacial marine deposits and the overlying and underlying foraminiferal ooze with its relatively warm-water fauna. As the areas of pack ice and bergs expanded southward glacial marine sediments began to accumulate where, not long before, the remains of warm-water pelagic organisms had been accumulating. So also, when the southern limit of the floating ice retreated the warm current followed it northward, showering the top of the glacial marine sediment layer with warmer-water Foraminifera and coccoliths. This mechanism is illustrated diagramatically in Fig. 2. Two significant inferences may be drawn from this diagram; the first is that the process is a continuously recording one; the second is that in cross section the layers of glacial marine sediment are wedge-shaped, thinning out and disappearing southward and thickening northward. The layer of foraminiferal ooze that overlies the uppermost glacial marine layer in the cores apparently must wedge out if it is traced far enough northward because a fine-grained type of glacial marine sediment is now forming in the polar region and, for all we know, has been accumulating there continuously since the last glacial epoch. Traced southward from the latitude of the cores the glacial marine zones should wedge out or grade out into another type of cool-water deposit—perhaps red clay or foraminiferal marl containing a cool-water fauna. It is suggested by Schott's recent work on the Foraminifera from the cores taken on the *Meteor* expedition (1925–1927) that the uppermost glacial marine zone of the Piggot cores may be represented in the equatorial Atlantic by a zone of sediment containing Foraminifera that indicate cooler surface water than now exists in the same locality. Schott interprets this zone as probably the tropical equivalent of the last glacial maximum.

This inferred distribution of the glacial marine zones in a hypothetical cross section of the sea floor along a meridian in the western part of the Atlantic between

latitude 20° and the north pole is shown graphically in Fig. 3. Only the study of additional cores that may be taken in the future can show how near or far from correct is this inference.

More than ordinary interest attaches to the interpretation of the glacial marine zones of these cores, because cores of ocean bottom sediments of this length and longer open a new approach to the study of glacial epochs. One of the first questions that arises is how much of the whole Pleistocene or glacial epoch do the glacial marine layers in the cores represent? On the land the history of the Pleistocene has been divided into several major glacial stages separated by warmer interglacial stages and each stage has been divided into substages.

We know that the whole Pleistocene epoch, which embraces four or more major glacial stages, lasted something like a million years or more. Antevs has estimated from his study of certain annually stratified or varved glacial sediments that the last ice sheet disappeared from North America only 10 to 15 thousand years ago. This interval since the last ice sheet disappeared is known as the postglacial interval. The postglacial sediment in these cores is that which overlies the uppermost glacial marine zone. In the four cores west of the mid-Atlantic ridge that contain four glacial layers the postglacial sediment averages only a little more than one foot in thickness. This means that only that much sediment has accumulated on the ocean floor in the past 10 or 15 thousand years. Then, if we make the reasonable assumption that the other sediment in these cores accumulated at a roughly comparable rate, it would mean that a 10-foot core represented probably not more than 150,000 years. Even allowing for large errors this seems rather clearly to be too short a time for the whole Pleistocene epoch. Consequently, we concluded that the four glacial marine layers in the cores more likely represent glacial substages of the last major stage.

One more bit of evidence suggesting that this is the more probable interpretation can be inferred from the sediment in the lower part of core number 7. Below the lowest glacial layer in this core is a layer of chalky sediment more than 3 feet thick that consists almost wholly of foraminiferal shells. But the intriguing thing is that the Foraminifera indicate water temperatures as high as, or higher than, those that prevail in that part of the Atlantic to-day. Here then is a layer that represents a climate as warm as, or perhaps somewhat warmer than, the present and, moreover, if the thickness of the layer means anything, a period of warm climate that persisted for perhaps three (or more?) times longer than all postglacial time. Such a long, warm interval suggests a major interglacial epoch; that is, an epoch between two major glacial stages.

I can not refrain, however, from commenting here on a highly speculative inference that one might draw from the interpretation suggested in the preceding paragraphs. That is, the composition, texture and thickness of postglacial sediment in the cores is more closely analogous to the sediment representing the intervals that separate glacial substages than it is to the much thicker layer of sediment that represents the last major interglacial stage. This raises the question whether we may not be living in one of those intervals that separate glacial substages rather than in a true interglacial epoch? The temptation to pursue this idea and perhaps be confronted with having to make a prognostication is effectively curbed by the recollection of Mark Twain's observation that "There is something fascinating about science. One gets such wholesale returns of conjecture out of such a trifling investment of fact."

Nevertheless, it seems to me that much could be learned from systematic coring and mapping of these interfingering wedges of glacial marine and warmer-water foraminiferal sediments. The

southern edges of the glacial deposits and the northern feather edges of the warmer-water deposits should, by comparison with the present ice limits and present climate, serve as reasonably good measures of the intensity of climatic changes that produced glaciation and deglaciation, or at least partial deglaciation. Also, the relative duration of these climatic pulses could be learned from the oceanic sedimentation because that process is a continuous and fairly regular one. Furthermore, adequate sub-oceanic maps of the southern edges of glacial marine deposits should enable us to draw pretty fair maps of the Gulf Stream across the greater part of the North Atlantic during the glacial maxima. Such charts of the Gulf Stream's deflected course might enable climatologists to reconstruct more fully and more accurately the climate of western Europe during the glacial maxima. A knowledge of the relative intensity and duration of these alternating cold and warm epochs might conceivably throw light on the migrations and cultural development of pre-historic peoples in Europe.

Submarine volcanic activity is suggested by the shortest and the longest cores of the group (numbers 10 and 11 of Fig. 1). The shortest core (number 11) is only 13 inches long. The upper 6 inches consists of limy foraminiferal ooze like that on the floor of most of the North Atlantic Ocean. Below this ooze is a light-colored, clayey mass that contains nodular lumps of the volcanic rock, basalt. These lumps of basalt are deeply altered—their constituent minerals have undergone a chemical change that deprived them of their original hardness, lustre and sharply angular form and reduced them to an aggregate consisting largely of minute flaky or micaceous mineral particles of clay. The intensity of the alteration diminishes inward toward the centers of the lumps so that their centers are composed of comparatively hard rock. At the bottom of the core, however, the rock is only moderately altered and therefore so hard that it stopped the core bit despite its very considerable velocity.

The clayey mass in which the basalt lumps are embedded resembles in texture and mineral composition the thin alteration rims found on the surfaces of small pieces of basaltic pumice and basaltic glass scattered through several of the other cores. This suggests that most of the clayey mass was once basaltic glass that formed by solidification of the lava when it came in contact with the sea water. Lava of that composition if cooled very slowly crystallizes and becomes a rock composed of an aggregate of interlocking crystals. Now because the upper surface of the clayey mass has numerous small cavities or vesicles that closely resemble the vesicular surface of certain lava flows, because the clay contains lumps of basalt, and finally, because, at the base of the core, the basalt is comparatively fresh we suggest that this core may have struck a lava flow which, several thousand years ago, was extruded on the ocean floor—a lava flow that in coming in contact with the cold sea water was chilled to a glass on the outer part. Doubtless the glass cracked and tended to shatter but presumably the continual flow of heat from the intensely hot interior kept the glass partly plastic for a long time.

Another observation that lends support to the hypothesis that the rock encountered is part of a submarine lava flow, is that the upper part of the clayey mass contains scattered grains of sand and Foraminifera shells. Our inference is that the turbulent water around the edge of the lava flow carried these things up from the sea bottom as the lava front advanced and dropped them into cracks and hollows on the lava surface so that they were, in effect, incorporated in the upper part of the glassy mass. More significant, however, is the fact that the original calcium carbonate of these occluded Foraminifera shells has been converted to another mineral—a silicate that

is probably the mineral phillipsite. Now phillipsite, although it has been found as well-formed crystals in deep-sea oozes, has not, so far as we are aware, been found replacing foraminiferal shells. On the other hand, phillipsite has been found in basaltic rocks in many parts of the world. It appears from all this that the chemical transformation from calcium carbonate to the silicate mineral was engendered by the exhalation of heat and hot solutions from the lava. It also appears to us that the thick mass of clayey material surrounding the basalt fragments must have been produced at an accelerated rate by the heat from the lava, as the small pieces of basaltic glass in other cores have only thin coatings of clayey material and imply that under normal conditions chemical reaction between (cold) sea water and basaltic glass takes place very slowly.

One more bit of evidence was found that also fits in with the hypothesis that the rock struck by this short core is a part of a submarine lava flow. Not a single shard of volcanic glass was found in the foraminiferal ooze overlying the clayey mass and basalt, whereas in all the other cores the shards are scattered through the upper parts, and sparsely, all the way to the tops. It may sound paradoxical to regard the absence of volcanic ash as suggestive evidence for a submarine lava flow, but the paradox disappears when we know that the shards of volcanic glass found in the upper parts of the other cores have a distinctly different chemical composition from that of basalt and that they could not, therefore, have been derived from a volcanic vent that was extruding basaltic lava. Our tentative interpretation is that the basaltic lava spreading over the sea floor at the site of core 11 sealed beneath it the sediment containing the scattered shards of volcanic glass so that they could not in any way be reworked into the later sediment that accumulated on top of the lava flow. Were this rock simply a boulder that had been dropped there by an iceberg it seems probable that successive generations of bottom-feeding animals would have worked some shards up into the uppermost sediment at this core station as they apparently did at all the others.

After all, however, we must bear in mind that this short core represents only a single point in the ocean abyss and, despite the fact that it contains several mutually consistent bits of evidence that the rock it encountered is part of a submarine lava flow, it will require several more cores in that immediate vicinity before the existence or non-existence of such a flow can be definitely established. There still remains the possibility that the rock encountered is the upper part of a very large boulder of basalt that was dropped there by an iceberg and that, by reason of the size, this upper part for a long time projected above the sea floor. Very long exposure to the sea water might have produced its deep alteration zone of clay. This interpretation, however, apparently leaves unexplained both the conversion of the foraminiferal shells to a silicate mineral and the absence of volcanic ash shards from the sediment that covers the rock; furthermore, the large boulders that have been dredged from the deep parts of the ocean, although not of rock types that are so readily altered, have been remarkably little affected by their stay in Davy Jones's locker.

The longest core of the group, number 10, is quite as anomalous as is the shortest core. It was taken several hundred miles west of the shortest core in 13,780 feet of water. At this station the coring device buried itself and an unknown amount of soft mud was lost out the top of the core barrel. However, we know from sediment that stuck to the ship's anchor flukes that the sea floor at this station is covered with normal foraminiferal ooze. This core, which is the full 10 feet in length, contains two zones of

highly distinctive mud—a homogeneous, very fine-grained, dark gray mud that shrank greatly upon drying. At the bottom of the core is a little more than 3 feet of this mud and at the top is another zone of it a little less than 3 feet thick (plus some more that was lost out at the top of the coring device). Between these two zones is normal foraminiferal ooze and glacial marine sediment.

These two relatively thick mud zones differ from all the sediment in the other cores in composition and content of organic remains. The mud in these zones consists of a mixture of minute clay particles and almost equally minute particles of basaltic glass and of the constituent minerals of basalt. The glass and mineral particles of basaltic composition make up approximately half of the mud. Foraminifera shells and the limy parts of other organisms are exceedingly rare or absent except in the upper part of each zone, where they become progressively so much more plentiful that mud of each zone grades upward into normal foraminiferal ooze. The abundance of basaltic mineral particles and fresh basaltic glass point to a volcanic source for much of the material and the scarcity of foraminiferal shells and coccoliths suggests that the peculiar mud accumulated in so short an interval that only a very few of these organic constituents reached the bottom.

Basaltic particles, comparable with those just described, presumably were discharged into the water of the Mediterranean over the Nerita Bank off the coast of Sicily during the submarine eruption of 1831, for according to H. S. Washington's account, " . . . the surface of the sea was seen to rise to a height of 80 feet, the column maintaining itself for 10 minutes, and then again sinking down. This was repeated every quarter to half an hour, and was accompanied by a dense cloud of black smoke and loud rumblings." The black smoke presumably consisted of basaltic dust particles. Other submarine volcanic eruptions have been observed in various parts of the world, notably in the Strait of Sunda off the coast of Java, but, of course, all in comparatively shallow water. It is our belief that two submarine volcanic eruptions on the ocean floor gave rise to the two zones or layers of basaltic mud in core 10. Such eruptions at great depths would presumably have no perceptible effects at the ocean's surface.

The volcanic vent from which this material came, however, apparently was not close to the site of this core. This is inferred from the fact that the clay and basaltic particles in the greater part of each mud zone make up a homogeneous mixture. No gradation in size of particles from large at the bottom to fine at the top is discernible. Had the material been thrown violently into suspension as a great cloud near the site of this core the particles would surely have fallen into a well-graded sequence with the heavier basaltic particles at the bottom and the minute flaky particles of clay above. As they are not so arranged we might speculate that the volcanic eruptions occurred at some distance from core 10. One possible interpretation is that submarine volcanic eruptions discharged finely divided basaltic particles into the sea and at the same time threw into suspension much clay derived largely from the deeply altered surface of earlier submarine lava flows. Such a mixture of material, having settled to the bottom, would, by reason of its fine grain, make a quite labile sediment that would flow readily, even on a gently sloping surface, and so would tend to collect in the hollows and deeper depressions on the sea floor. Our speculation is that the basaltic mud moved to the site of core 10 as submarine mud flows that resulted from more or less remote submarine volcanic eruptions. The gradation upward from basaltic mud into the overlying foraminiferal ooze we may attribute to the incessant work of mud-feeding animals

that subsequently established themselves on the surface of the mud flows. Indeed, in the upper part of the upper basaltic mud layer of core 10 there are numerous mud-filled burrows, tubes and coprolitic lumps that evidently were made by mud-dwelling animals.

III

Certain purely geologic evidence found in these cores may be of significance for physical oceanographers in their study of the circulation and dynamics of ocean water masses. As revealed by these few cores, currents apparently move rather rapidly across the crest of the mid-Atlantic ridge. There, nearly all the finest mineral particles, the smallest shells and diatoms, and the lightest shards of volcanic glass have been swept away, leaving the heaviest foraminiferal and pteropod shells and the coarse sand. The mid-Atlantic ridge is a veritable mountain chain that extends southward from Iceland through both North and South Atlantic Oceans dividing them longitudinally into two approximately equal basins. Where the line of these cores crosses the ridge it is known as the Farady Hills. These rise about 8,000 feet above the general level of the ocean bottom on either side, yet their tops are 4,200 feet below the ocean surface. Perhaps this great ridge accelerates the flow across it by constricting the cross section of a large volume of slowly moving water.

The next core east of the mid-Atlantic ridge contains, in contrast with all the other cores, a great abundance of small diatoms, coccoliths, minute shards of volcanic ash and clay particles. The abundance of these in this deeper-water core, their absence from the core on the ridge and the entirely normal texture of the core just west of the ridge suggest, but do not prove, that the current moves across the ridge from west to east. More cores in the vicinity of the ridge are necessary to demonstrate whether or not this interpretation of current direction is valid.

Currents of comparable velocity move across the outer edge of the continental shelf off the southwest coast of Ireland. The easternmost core of the series taken there in water 6,420 feet deep consists predominantly of large foraminiferal shells and coarse sand and gravel. It resembles rather closely the core taken on the top of the mid-Atlantic ridge and, to geologists, the explanation for the texture of both must be the same. We know of no other agent than currents of water that will so effectively winnow out the fine from the coarse in bottom sediments.

Having looked a little way into the past that has been revealed by this exploratory series of North Atlantic deep-sea cores, we may now look ahead and consider what sort of information is to be expected from the long-core method of studying sediments not otherwise accessible. Although information that may be obtained from deep-sea cores will perhaps be used primarily for geological and geochemical investigations, it seems that the method has a broader scope and may yield data that are of value to climatologists and archeologists. Pelagic or surface-dwelling Foraminifera, as contrasted with those that live in the abyssal deeps, are reliable indicators of surface-water temperature and therefore, indirectly, of warm or cold climate. Obviously, however, their usefulness is restricted to the thermal element of climate for they can tell us nothing of the wetness or dryness of an epoch. Significant evidence bearing on postglacial climatic changes might be obtained from detailed study of the Foraminifera in cores taken in parts of the ocean where postglacial sedimentation has been comparatively rapid as, for example, near the seaward edge of the blue mud zone. In this zone, or belt, sedimentation is more rapid than it is farther seaward because it receives considerable clay from the land in addition to the normal sup-

ply of the remains of pelagic organisms. The object of selecting the seaward edge of the blue mud zone for this kind of investigation is to strike places where the postglacial sediment is as nearly as possible of the same thickness as the total length of the cores. On the assumption that the sediment near the seaward edge of the blue-mud zone accumulates at an essentially uniform rate, the climatic fluctuations found would be approximately located in time within the postglacial interval. It is conceivable that by such a method climatic fluctuations could be correlated from place to place along the ocean margins from the Arctic to temperate or even tropical latitudes and perhaps also from continent to continent.

Long cores of sediment from the shallower straits between continents and islands may perhaps yield reliable evidence of former land bridges. One might, for example, anticipate finding swamp deposits or other similar features below marine sediments in cores taken in the shallow waters of Bering Strait.

Cores of the deep-water sediments in the Mediterranean Sea should contain an unusual amount of information because the peculiar hydrography of the Mediterranean makes it rather sensitive to climatic changes and because the Mediterranean region is one of volcanic and seismic activity.

The pelagic fauna of the Mediterranean was presumably quite different at the end of the last glacial stage because of the greater volume of fresh or feebly saline water that flooded its surface. The more or less gradual change from that condition to the present hydrographic condition should be marked in the sediments, especially of the western basin, by the appearance of generally Atlantic types of coccoliths, pteropods and Foraminifera which inhabit the surface waters now. Indeed, these forms may have reached a recognizable peak of abundance during the higher sea level of the "climatic optimum." At some level, perhaps corresponding to late Neolithic time, when, according to Sandford and Arkell, northern Egypt became desert, the wind-blown sand from the region south of the Mediterranean should begin to make its appearance. This wind-blown sand is abundant in the sediments forming to-day in the deep basins of the Mediterranean.

Less vague, however, would be the record left by the activity of explosive volcanoes. A considerable number of these ash falls in the upper part of the post-Pleistocene column of sediments should be correlatable with human history. Earthquakes, too, should have caused submarine mud slumps that threw into suspension much sediment which settled as wide-spread blankets of distinctive sediment. Such a blanket of sediment should show a gradation in grain size owing to the differential settling rates of the constituent particles thus thrown temporarily into suspension.

It seems probable, therefore, that long cores of the sediments in the deep basins of the Mediterranean would reveal an extraordinarily rich and varied record in a locality critical not only by reason of the unusual configuration of the basin but also by reason of the wealth of information that is already available from the long historic records, the archeology and the Pleistocene and post-Pleistocene geology.

The long cores from the North Atlantic, which penetrated older oceanic strata than have ever before been brought up from the deeps, are, in a sense, pioneers in a new and vast field of oceanic geology. Their study has revealed fragments of late geologic history that we may expect to be enlarged and fused into a composite whole as more and more cores are taken in systematic surveys of the ocean floor. So vast is this field to be explored that the cores already studied are but pin points of light in an abyss of darkness.

WILLIAM BARTON ROGERS, PIONEER AMERICAN SCIENTIST

By Dr. ARTHUR BEVAN
STATE GEOLOGIST OF VIRGINIA

WILLIAM BARTON ROGERS was a pioneer American scientist, living during the third century of the American colonization. During the span of his life, 1804–1882, he made fundamental investigations and discoveries in his chosen field of geology and he pioneered in other fields. He was also a lucid and inspiring teacher, an eloquent and captivating public lecturer and a sagacious and masterly organizer and administrator. In him were combined the qualities of a pleasing personality, devotion to pure and practical science and a zeal for effective public service.

To Virginians and others, his life will be of perpetual interest because he organized the first official geological survey in the Commonwealth, produced its notable accomplishments almost single-handed, and made lasting contributions to the historic achievements of the College of William and Mary and the University of Virginia. By scientists and engineers everywhere he will be venerated for the practical fruition of his vision of the Massachusetts Institute of Technology. By members of the American Association for the Advancement of Science—present and future, scientists and laymen—he will be esteemed as one of its founders and principal leaders. He will also be remembered as one of the early presidents of the Association, and he was the third president of the National Academy of Science.

To evaluate the accomplishments of Rogers as a geologist, it will be helpful to review briefly conditions in his time from various points of view: One should have in mind the state of geology then as a science and its public status in Virginia and elsewhere; the difficulties with which Rogers had to cope in pursuing his researches and performing his official tasks; and his coordinate activities as an educator, organizer and administrator, because the manifold activities of the versatile man were closely related. The immediate ancestry, home training and education of Rogers had also an important directive influence upon his productive life. The stage upon which he moved was constantly changing, but he created opportunities and used his talents to develop them for the general welfare.

SKETCH OF LIFE

William Barton Rogers was born in 1804 in Philadelphia of Scotch-Irish parentage, his father having been expatriated from Ireland. His father became a physician before William was born and continued to practice medicine until he was appointed in 1819 to the chair of chemistry and physics at the College of William and Mary in Virginia. Here, William received his college education. The classics, chemistry, mathematics and natural philosophy were his chief fields of study, but he is said to have excelled especially in mathematics. His father, apparently gifted as a teacher and with an absorbing interest in the education of young men, imparted much knowledge and wisdom to his sons. William's own talents and ability soon became so evident that at the age of twenty-three years he was lecturing in

Baltimore on natural science and teaching natural philosophy at the Maryland Institute in Baltimore. Then for seven years he taught chemistry and natural philosophy at the College of William and Mary as successor to his father, following which he was appointed to the chair of natural philosophy at the ten-year-old University of Virginia. John Stewart Bryan, president of the College of William and Mary, states that "his wide and practical imagination was displayed [at William and Mary] by his insistence that something be done for the study of geology in Virginia." In 1833 he was elected a correspondent of the Academy of Natural Sciences in Philadelphia. He was thirty-one years old when he became, in 1835, the first state geologist of Virginia. In 1853 he moved to Boston, where after some time he organized what is now the Massachusetts Institute of Technology.

Influence of Henry D. Rogers

William's brother Henry, who was four years younger, at the age of twenty-five years, in 1833, was elected a fellow of the Geological Society of London. During a year in London, Henry associated with the leaders—and they were giants—in British science, and kept William informed about the progress there in the development of the new science of geology. Soon after Henry's return to the United States, the brothers began jointly field studies in the Appalachian region. In 1835, the year that William came to Charlottesville, Henry was appointed professor of geology and mineralogy at the University of Pennsylvania and also chief of the newly organized state geological survey of New Jersey. A year later he was also placed in charge of the first geological survey of Pennsylvania. In these positions, with concurrent tenure through 1839, he made epochal contributions to the analysis and interpretation of the complex structure and stratigraphy of the northern Appalachian region. William, in Virginia, participated in some of these far-reaching investigations. Thus the scientific and official careers of these two gifted brothers were for several years developing along closely parallel lines of mutual interest and work.

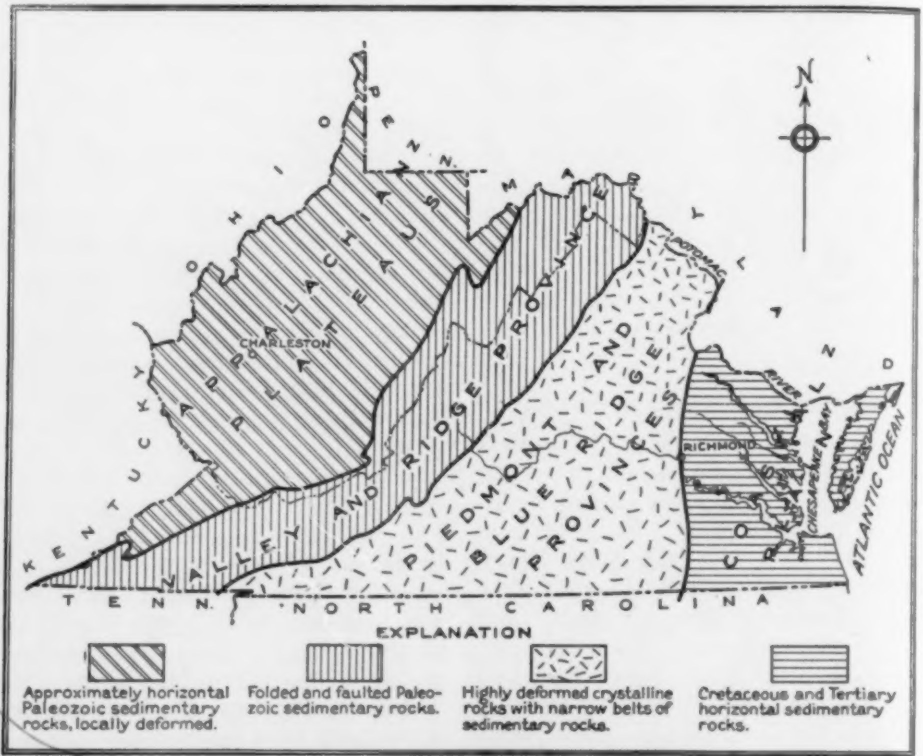
Geology in Great Britain

Henry D. Rogers' visit in England was at a time of revolutionary developments in geological theories. Numerous surmises of surprising accuracy about geologic phenomena had been made now and then during the centuries prior to 1800, but sound observations and correct interpretations were relatively rare among all the speculations, many of which were essentially fortuitous. As stated by Geikie,[1] a turning point in geological observations and theories was reached when James Hutton, a Scottish chemist, physician and gentleman farmer, after years of careful observation of earth features in many regions, aided in the founding of the Royal Society of Edinburgh in 1783. Two years later he presented before the society his "Theory of the Earth." In amplified form, this monumental work was published in 1795, but it remained almost unknown until cogently interpreted by Playfair, in 1802, in his classic "Illustrations of the Huttonian Theory of the Earth." Because of the "precision of statement and felicity of language" that made Playfair's volume without "superior in English scientific literature," this work soon came to be regarded as the foundation of English geology.

Playfair's treatise was being widely read at the time the Rogers brothers were receiving their education and acquiring their tastes for careers in natural philosophy and geology. Further stimula-

[1] Sir Archibald Geikie, "The Founders of Geology," p. 295. New York, 1905.

tion no doubt was afforded by the publication in 1830—when William was 26 years old—of Lyell's epochal "Principles of Geology," soon to become the textbook par excellence in geology throughout the English-speaking part of the world. It was also destined to be widely read and used for many decades. Through it, many geologists became indoctrinated with a cardinal principle of decades, or even the major part of the century since William Barton Rogers made the first official survey of Virginia's geology and mineral resources. Although the guiding philosophy of geology was being formulated in Europe and the early steps in its application to the American scene were in progress, "none of the sciences was taught in the colleges and other institutions of learning



THE VIRGINIAS IN ROGERS' TIME

THE AREA MAPPED BY ROGERS COVERED ABOUT 65,000 SQUARE MILES AND LIES IN FIVE GEOLOGIC PROVINCES, CONTAINING A GREAT VARIETY OF GEOLOGIC FORMATIONS AND STRUCTURES.

geologic philosophy—that the present is the key to the past.

Geology in the United States

The stage upon which the Rogers brothers together played their roles as leaders in a sphere of scientific thought was very different from that of recent

in America or England"[2] at the time of Rogers' birth. Merrill has stated also that "the general trend of public opinion was decidedly against the study of geology."

[2] G. P. Merrill, "The First One Hundred Years of American Geology," p. 23. Yale University Press, 1924.

Jefferson, Silliman, Mitchell and others were nonetheless making at that time definite contributions to the science of geology in the United States. Several books—textbooks and others—and geologic maps had been published during Rogers' formative years by such aggressive pioneers as MacClure, Cleaveland, Eaton, Schoolcraft, Hitchcock, Emmons, Hayden and a few others. Less than a dozen articles had been published on the geology of Virginia by the time Rogers really became interested in it. Most of them described conspicuous local features, such as the Natural Bridge, the Natural Tunnel and a part of the Richmond coal basin. Still earlier Jefferson had stimulated some interest in the geology of Virginia by his paleontologic studies, continued even during his executive career. His report[3] on the discovery of gold added to the local enthusiasm. Discussions of regional problems or fundamental principles of interpretation, however, were still lacking.

Rogers had published several scientific papers before he was appointed state geologist and professor of natural philosophy at the University of Virginia. His interest in geology was first publicly evinced by a paper on artesian wells that appeared in 1834 in the "Farmer's Register" of Richmond. He discussed such topics as greensand, analyses of shells and calcareous marl; in all of which he gave evidence of great ability in making broad generalizations from observations.

—*J. K. Roberts*

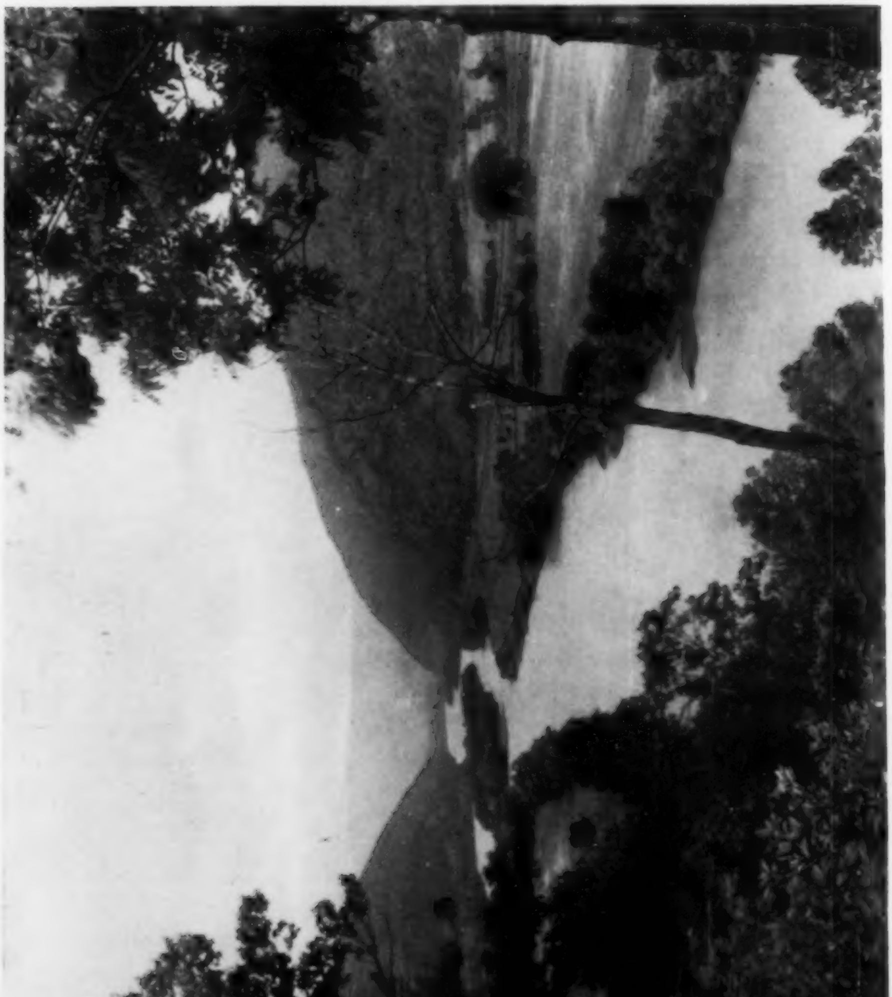
SHELL BED IN YORKTOWN FORMATION

A FAMOUS COLLECTING PLACE FOR TERTIARY FOSSILS ALONG YORK RIVER BELOW YORKTOWN.

Early Geological Surveys

It is not recorded just when some one first publicly advocated the need of an official geological survey to determine the basic geologic conditions in a particular state and to aid in the development of its natural resources. The idea, however, was the natural and inevitable outcome of the extensive private field investi-

[3] Thomas Jefferson, "Notes on the State of Virginia," 1786.



NARROWS OF NEW RIVER IN GILES COUNTY, VIRGINIA

A TYPICAL GORGE CUT THROUGH THE WESTERN RIDGES OF THE VALLEY AND RIDGE PROVINCE.

gations in the seaboard states. By 1830 significant work, pregnant with possibilities, was in progress, particularly in New York State, New England, Canada, North Carolina, Tennessee and Virginia.

The North Carolina State Board of Agriculture, in 1823, authorized the making of a geological survey of the state, as a result of which a report was published in 1824. Another early attempt to establish an official state geological survey was made in 1824, when the General Assembly of South Carolina appropriated $1,000 for the "salary of the Professor of Geology and Mineralogy . . . and $500 for making a geological and mineralogical tour during the recess of the college, and furnishing specimens of the same." It has been said that little of importance resulted from this venture, but a report on it was published in 1826 in the newspapers of the state. Eaton's survey of Rensselaer County, New York, in 1821, was, according to Merrill, "the first sufficiently thorough and systematic survey to be dignified as a geological survey," but that work was done with private funds.

The decade 1830–1839 was most important in official investigations by states of the geology and mineral resources within their respective borders. During that period, fifteen state geological surveys were organized first in Massachusetts, then in Tennessee and Maryland, prior to 1835, when the first geological survey of Virginia was authorized.

The Virginia Historical and Philosophical Society, of which John Marshall was the first president, was established in 1831. Among its objectives was the development of the agricultural resources and the study of the geology of the state. Gold mining from 1829 or slightly earlier no doubt gave impetus to the movement to survey the mineral resources. Coal had been mined in the Richmond basin for three-fourths of a century. Peter A. Browne, corresponding secretary of the Geological Society of Pennsylvania, in 1833 wrote from Philadelphia, urging consideration by Governor Floyd of the importance of establishing a geological survey in Virgina. W. B. Rogers, then a professor in the College of William and Mary, was an

CHARACTERISTIC TOPOGRAPHY IN VALLEY AND RIDGE PROVINCE
A REGION OF VALLEYS AND HEAVILY WOODED RIDGES, LITTLE SETTLED IN ROGERS' TIME.

—Rhodes

RUGGED BLUE RIDGE IN SHENANDOAH NATIONAL PARK

A REGION OF COMPLEX GEOLOGY ALMOST INACCESSIBLE IN THE TIME OF ROGERS, BUT MAPPED BY HIM.

ardent advocate of the step. In this letter to Governor Floyd, Mr. Browne wrote many such stirring sentences as, "In the beautiful and flourishing city of Richmond, I observed the fronts of two stores fitted in the new and fashionable style with granite (so called), (sienite) from Massachusetts, while there exists in the James River and on its banks, in the immediate vicinity of the town, rocks of a superior quality, in quantities amply sufficient to build a dozen cities."

Governor Floyd in his message to the General Assembly, among other reasons for a geological survey, stated, "It is well known that Virginia affords, perhaps, the most extensive mines of iron of any other country of the same extent, and fine specimens also of gold, lead, copper, plaster of paris or gypsum, and fine inexhaustible mines of bituminous coal. . . ." It should be noted that the enormous deposits of coal in the western part of Virginia, then including West Virginia, had not yet been discovered.

The act of the General Assembly, passed on March 6, 1835, provided for the appointment of " a suitable person to make a geological reconnaissance of the State . . . and to report to the next general assembly a plan for the prosecution of a geological survey of the State." William Barton Rogers, professor of chemistry and natural philosophy at the College of William and Mary, was selected as the "suitable person" to formulate plans and to inaugurate the new enterprise.

Rogers as State Geologist

Rogers went to work soon after his appointment to survey carefully and thoroughly all of the Virginias, then embracing about 64,500 square miles. It is doubtful whether even those who have reconnoitered in untraversed lands can

WARM SPRINGS VALLEY, VIRGINIA
A LONG, NARROW LIMESTONE VALLEY ERODED IN A LARGE ANTICLINE, FLANKED BY SHARP SANDSTONE RIDGES.

fully appreciate the manifold handicaps, some almost insuperable, under which Rogers labored and triumphed. His "clear and penetrating intellect, . . . ready and retentive memory, . . . sound judgment, . . . observant eye, rich and soaring imagination," and habits of "painstaking thoroughness . . . even in matters of seemingly insignificant detail" equipped him well for a pioneer understanding of such magnitude.

Rogers had no real base maps upon which to plot his observed data or to guide him in the field—not even the "old reconnaissance topographic maps" which we are prone to decry in this age of accurate large-scale maps showing details of the terrain. His assistants were untrained, but being sharp observers of natural phenomena he coached them into useful helpers. Transportation was slow and arduous, for much of the terrain was rugged and a large part of the vast territory was unsettled, uncharted and unknown. Overshadowing all, was the factor of limited time—only six years eventually available by annual appointments to survey almost 65,000 square miles during the time that could be spared from exacting professorial duties at the University of Virginia.

In the face of conditions that might well make a geologist of today, with all modern facilities, pause for ample consideration before accepting such an assignment, Rogers went to work so assiduously that he was able to submit material for seven annual reports before the appropriations ceased. He prepared also much of the data for a large geologic map of the Virginias, accompanied by ninety-six detailed structure sections which were published in 1884, with a small-scale generalized geologic map of the Virginias that Rogers had once made for Major Jed. Hotchkiss, of Staunton, Va.—all appear-

ing in a posthumous reprint of the annual reports.[4] Rogers had thought for a long time after the Geological Survey was discontinued that funds would be provided for the publication of a comprehensive final report, including his original geologic map. Hence, the brevity and general character of the annual reports should not be too critically judged. But, as Merrill states, they "are all there is to show for years of careful and patient work under most adverse circumstances."

the widely expanded floor of an ancient ocean. Massanutten Mountain, the bold medial "range" lengthwise of northern Shenandoah Valley, was recognized as being a great synclinal tract resting in a trough of shale—an observation that has been improved only in regard to details.

Rogers also observed "the extraordinary phenomenon of inversion" along North Mountain, at the northwest side of Shenandoah Valley, whereby the beds apparently are conformable but in reversed stratigraphic position. He cooperated with brother Henry in preparing, in 1842, a most important descriptive paper "On the physical structure of the Appalachian chain." Merrill stated that "this work, so far as it related to the structure of the chain, has been improved upon in recent times only by the discovery of enormous overthrust faults in the southwestern portions." In the same year William read a paper on thermal springs in Virginia in which the geologic environment of many of the springs was correctly interpreted.

BURKES GARDEN IN SOUTHWESTERN VIRGINIA
A BROAD, SECLUDED BASIN ERODED ON LIMESTONE, ENCIRCLED BY HIGH SANDSTONE RIDGES.

Geologic Contributions

Rogers made the first classification of the Paleozoic sedimentary rocks, some 40,000 feet thick, west of the Blue Ridge in Virginia, dividing these strata into fourteen groups which he designated by Roman numerals. He inferred from the conformity of strata and their succession and general physical characteristics that they were part of a great series of formations that had accumulated over

[4] A reprint of annual reports and other papers on the geology of the Virginias, by the late William Barton Rogers; New York, 1884.

THE IRON GATE ARCH, ALLEGHANY COUNTY, VIRGINIA
JACKSON RIVER HERE HAS CUT ACROSS A LARGE ANTICLINE IN SILURIAN SANDSTONE.

SHENANDOAH VALLEY, VIRGINIA
LOOKING ACROSS THE EASTERN HALF OF THE VALLEY BETWEEN THE BLUE RIDGE IN THE FOREGROUND AND MASSANUTTEN MOUNTAIN. THE VALLEY FLOOR IS PART OF AN EXTENSIVE PENEPLAIN.

Bailey Willis has recently characterized the work of Henry and William Rogers on structure thus: "The Appalachian structure . . . was fully described in a manner that has not been bettered in four-score years, by the Rogers brothers. . . ." Much of this work was done by the brothers after tions to the basement crystalline rocks. In his annual report for 1839, the Tertiary marl south of the James was described in detail and its Eocene and Miocene ages were noted. In the report for 1840 he identified some of the Miocene fossils. He also reported the discovery of a "remarkable stratum varying from

DISSECTED APPALACHIAN PLATEAUS IN WESTERN VIRGINIA
THE "BREAKS OF THE SANDY." A TYPICAL GORGE CUT IN THE SANDSTONE PLATEAU.

William ceased to be state geologist in 1842, but while he was teaching at the University of Virginia. Henry was responsible for many of the principles formulated; William contributed many observations from his remarkable field survey of the Virginias.

Rogers classified the Coastal Plain formations according to their periods of deposition and also recognized their rela- 12 to 25 feet in thickness, composed almost entirely of microscopic fossils and lying between the Eocene and Miocene." This was the first recorded discovery of diatomite in the United States.

He made studies of New England geology, even before he moved to Boston, in 1853, in order to be able to devote more time to original investigations. As early as 1846 he stated that the site of the

White Mountains had been covered by the sea during Silurian time, a deduction obviously based on the occurrence of fossils. On the other hand, he advocated in 1848 that certain glacial boulders could not have been transported by glaciers or icebergs but must have been carried over the high ridge crests by a catastrophic inundation from the Arctic Ocean! The physics of the supposed phenomena was discussed at some length. In a discussion before the Boston Society of Natural History in 1860, he stressed principles of

ROLLING LIMESTONE COUNTRY IN TAZEWELL COUNTY, VIRGINIA
BROAD VALLEYS HAVE BEEN ERODED ON THE LIMESTONE AND SHALE BETWEEN THE HIGH RIDGES CAPPED BY SANDSTONE.

stratigraphic paleontology that are currently accepted. During this year also he argued, in opposition to Agassiz, that the strata in New York State had accumulated in relatively shallow waters on a slowly subsiding sea floor. The discussion continued through two meetings of the society, with the result that Agassiz was at least partly convinced of the correctness of Rogers' view.

Rogers as a Teacher

William Barton Rogers began his professional career as a teacher, by starting with his brother Henry, at twenty-two, a private school near Baltimore. He also lectured at the Maryland Institute, where he soon became professor of natural philosophy. Upon his father's death, in 1828, he went to the College of William and Mary to occupy the vacant chair of chemistry and natural philosophy. After seven years he accepted appointment to the chair of natural philosophy at the University of Virginia which he held until 1853, when he resigned to move to Boston. From the beginning of instruction in the Massachusetts Institute of Technology, he taught physics for a few years until administrative duties forced him to relinquish that chair.

Rogers was a great teacher whose lectures were eagerly attended by large numbers of students. At the centennial of his birth his ability as a teacher was thus described: "In power to make difficult things plain, he was unequalled by any other teacher I have ever known. His capacity for luminous exposition was really extraordinary. . . . At his touch complex subjects became simple and

CHARACTERISTIC MOUNTAIN SLOPE IN SOUTHWESTERN VIRGINIA
THE RIDGE IS HELD UP BY FIRM SANDSTONE. THE SLOPES ARE ERODED ON SHALE. THE AREA WAS HEAVILY WOODED WHEN ROGERS DID HIS WORK.

dark things bright." Part of this skill came from his early discovery, as stated in 1829 in a letter to Henry, that "lecturing is in some respects to be considered as an art."

Science Associations

Absorbed as William Barton Rogers was in the art of teaching, in the discovery of new geologic truths in the field and in the practical applications of chemistry and physics, he found the energy and time to contribute positively and permanently in numerous other ways to the advancement of science throughout the nation. With his brothers he was a motivating spirit in the formation of the Association of American Geologists and Naturalists in 1840. The time was ripe for such an organization and the field was clear, for the American Geological Society, formed in 1819 at Yale University, had lasted only a decade. These earnest and farsighted efforts of the Rogers brothers soon bore fruit of undreamed worth. They led on September 24, 1847, to the permanent organization of the American Association for the Advancement of Science, directly from the group organized only seven years earlier. The association in turn was the parent of many leading societies, each devoted to the scientific interests of specialists in particular fields.

Although William B. Rogers has at times been given the additional honor of the first presidency of the association, it appears from the record that he was chairman of the organization meeting, as well as of the first meeting of the association held in Philadelphia the following year. William C. Redfield was chosen the first president. Rogers was president of the association in 1876. Three years later he was elected the third president of the National Academy of Science.

It is probable that the sojourn of Henry Rogers in Europe during 1831–33, which had considerable influence upon William's career as a geologist, initiated or helped mature the idea of a general group of American scientists, for the British Association for the Advance-

PARALLEL RIDGES IN VALLEY AND RIDGE PROVINCE, WEST VIRGINIA
THESE RIDGES WERE FORMED BY THE EROSION OF TILTED SEDIMENTARY ROCKS ON THE FLANKS OF ANTICLINES AND SYNCLINES. THE VALLEYS ARE ON WEAKER ROCKS.

ment of Science was organized during that time.

William participated, often with Henry, in the activities of numerous other scientific societies. He had early been made a correspondent of the Philadelphia Academy of Natural Science and a member of the American Philosophical Society. In 1842, he was elected an honorary member of the Boston Society of Natural History. William and Henry were unanimously elected foreign members of the Geological Society of London in 1844.

Massachusetts Institute of Technology

The last, and in some ways the most fruitful, epoch of Rogers' life was concerned with the founding of the Massachusetts Institute of Technology. His zeal for effective public service through the applications of science and his deep interest and broad insight into contemporary problems of education gave rise to a vision that did not long remain far from the realm of the practical.

In a letter in March, 1846, to Henry, who had written from Boston in regard to suggestions for expansion of the Lowell Institute, William at length formulated plans for a Polytechnic School in Boston. He was so imbued with the idea that later in the same day he sent another long letter in which some of his views were further developed. He stated, for instance, that "the true and only practicable object of a Polytechnic School is . . . the teaching, not of the manipulations and minute details of the arts, which can be done only in the workshop, but the inculcation of all the scientific principles which form the basis and explanation of them, and along with this a full and methodical review of all their leading processes and operations in connection with physical laws."

In 1862 the Institute was opened for classes, with Rogers as its first president and also as professor of geology and physics. So thorough had been his foresight, so painstaking his plans, so methodical his moves, that it was stated twenty years later that two remarkable

facts about the Institute could only thus be explained: the adaptation of the Institute to the public need and the general plan had required almost no modification.

Rogers, in spite of ill health that caused temporary retirement, remained the devoted guide of the Institute and one of its energizing spirits. As President Walker said in presenting him to the graduating class on May 30, 1882, only a few minutes before he died suddenly while delivering the address: "Founder and father is his title perpetual, by a patent indefeasible."

William Barton Rogers, first state geologist of the Virginias and the first teacher of geology in the Commonwealth, blazed the trails and laid the foundation for much of the later geologic research—pure and applied, official and private—in Virginia. He should thus be given appropriate rank among those pioneer scientists who built so well with intuitive skill and painstaking labor the broad base of our modern knowledge of Appalachian geology and the natural resources of the Virginias. Rogers also merits high rank as a preceptor and educator as well as an organizer and executive in other fields of science, particularly in applied chemistry and physics. His great monument is the Massachusetts Institute of Technology, but he occupies a large niche in the hall of fame associated with the creation and establishment of the American Association for the Advancement of Science.

As a result of the achievements of a talented and versatile pioneer Virginia scientist, it is no wonder that Austin Clark has recently said, "In view of his outstanding services to the State it is quite fitting that Virginia's highest mountain should be named for him." It is equally appropriate that this lofty summit should be an enduring reminder of the service of William Barton Rogers to the nation.

NATURALISTS IN THE WILDS OF BRITISH COLUMBIA

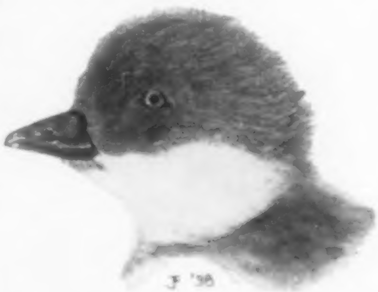
II. THE ENDING OF WINTER AND THE COMING OF SPRING

JOHN F. and THEODORA C. STANWELL-FLETCHER

GERMANTOWN, PENNSYLVANIA

JANUARY weather was colder on the whole than any other month. For outdoor work we were well clothed, wearing blanket parkas, heavy woolen shirts, warm, thick trousers, woolen socks and moose-skin moccasins. We used two or three pairs of mittens during the course of a day, as the perspiration soaked them, and they became cold and stiff when taken off for a few moments.

A lone white man, a fur trader, lived at certain periods of the year some thirty miles to the north of us on Bear Lake, where he traded with the few local Indians and other tribes from further north. Through an Indian, Chief Bear Lake Charlie, we learned that during this month the trader was expecting a plane to bring him mail and supplies. If we could reach his cabin in time for the plane we might for the first time in many months be able to send out mail and specimens for the museum. We therefore hired Charlie with his dog team to help carry our gear and guide us through the snow-laden and ice-bound valleys to Bear Lake. Bear Lake Charlie was a tall fine man, with graying hair, who always wore a bandana tied around his head and only needed large ear-rings to look the perfect pirate. Charlie had a large family, and when he tried to count his numbers of children he invariably became confused. He had seen much of life and knew well the ways of the wilderness creatures. He was shrewd, could lie with ease and unconcern, and put great store by the large crucifix which he always carried around his neck, suspended by a much-used piece of silk. From the trader he sometimes bought dried fruit, peaches, apricots, prunes and apples, and with these he made home-brew. He did not *sell* it to the other Indians; he merely invited them to join him with a cup or two and when they were settled in his shack he would lean against the door and tell his "guests" how much money the brew had cost, how hard his wife Selina had worked in preparing it, and how hard he had hunted and trapped to get money for buying the necessary ingredients, all in order that he might give his "brothers" a present. Before his guests departed they were shamed into contributing a little as a present also.



The trip to Bear Lake was tiring; deep fresh snow made the going heavy, and late on the first night we were obliged to camp, with a strong bitter wind blowing across the fifteen-mile lake, in Charlie's cabin. We were seven miles from the settlement and very footsore, having broken trail all day for the dogs and toboggan which followed, pushed and guided by Charlie. The wind whistled through the cabin, and the one candle guttered. We sacrificed a precious blanket to hang over the apology of a door. With Charlie and his two sons, aged fifteen and sixteen, who were helping him, we crouched by the tin stove which roared and crackled as the pitchy logs were fed to it. After supper, in the dim

light, Charlie held the family prayers with much gusto and, we fear, entirely for our benefit. His dogs outside snarled at each other, and it was a risky business for us to step out into the midst of them. The Indians made their beds on one side of the stove and talked together in their own tongue while we lay silent on our side. After a time the candle went out, our fresh bed of spruce branches, laid on top of the floor debris of years, was comfortable. There were a few snores and mutters—and it was morning.

INDIAN AND DOG TEAM ON DRIFTWOOD RIVER. MARCH, 1938.

At a little after noon we reached the settlement. The fur trader, a German, lived a lonely life and gave us a hearty welcome. His cabin, delightfully clean after the night in old Charlie's, had two small rooms, one his living-room, the other the store where he carried on his trades. We slept on the store floor at night, listened to up-to-date news on his radio, and worked over our mail and museum specimens in the daytime. We spent a week there and became a little acquainted with the Indians of the village. On the eve of our departure a boy came from the village, which was a quarter of a mile from the trader's store, and presented us with a soiled leaf from a notebook. On it was written in pencil "Dance TONITE at 7–30 P.M. By S. A. Wright." This was an invitation to a gathering in our honor. No white lady as far as we could find out had visited these people before, and they wished to show their appreciation. Armed with a few boxes of chocolate bars, we arrived in the middle of a fox-trot at the freshly scrubbed shack. We learned later that one of the Indians who had been "outside" had picked up this new dance and introduced it on his return. Gay young bucks, dressed in shirts with collars and ties, danced in their moose-skin moccasins with flamboyant squaws of various ages. One fine-looking chap had a light green scarf tied around his waist with a straight hair comb of the same shade stuck in his black hair. When the dance was well under way old Charlie came in with Selina, who carried a tin can which

TRACK MADE BY TWO WOLVES BESIDE SNOW-SHOE TRACK OF MR. FLETCHER.

she placed on the floor as Charlie's private spittoon.

The orchestra was made up of a violin, a guitar and an accordion. We applauded, listened to a lengthy but courteous welcoming speech given by the master of ceremonies, and replied by a speech ourselves of equal length. An hour later, at our request, we were given an exhibition of an old Indian tribal dance by two modern-looking couples. But it was obvious that the white man's fox-trots were considered as the proper thing and more to their taste.

It was during February that the wolves came. Their tracks were everywhere, and often they came within a hundred yards or so of our cabin. They were huge creatures, for a single footprint often measured six inches across, and skins, which we later saw at the Hudson's Bay Post, were seven feet from nose tip to tail tip. Occasionally we heard them in the distance. The sun was swinging higher each day and with the long hours of sunshine and preponderance of clear days, the snow melted and packed during the daytime to freeze at night when the thermometer still went down to twenty or thirty degrees below zero Fahrenheit. This made traveling on top of the hard crust pleasant for us, and the larger, heavier animals, like wolves, coyotes and moose, appeared again from other parts of the country where traveling conditions had been better during the early winter months. Whenever there was a fresh powdering of snow, the forests, swamps and meadows were laced with tracks of mice, shrews, squirrels, weasels, hares, mink and marten, wolves and coyotes. The moose stuck to swamps and the river banks, where they fed largely on young willow shoots.

One clear night at the full of the moon, after a fresh fall of snow, with the mercury at twenty-four degrees below zero Fahrenheit, we took a snow-shoe tramp at midnight through a fairyland of snow-covered trees and shadows, to a hill from the top of which there was a particularly fine view up and down the whole Driftwood Valley. It was light as day, and the stillness of the forest, mountains and lakes below us glittering in the moonlight

VIEW OF DRIFTWOOD VALLEY. FEBRUARY, 1938.

was thrilling as only a northern night can make it. Nothing either of us had ever seen in tropical places of the world or elsewhere could compare in sheer beauty with the nights of a northern winter. As we stood as silent as the wilderness around us, a lone wolf somewhere below us began to sing. Its musical voice, low and modulated, rose gradually to a high note and then as gradually died away, only to rise again and again. It was so full of sadness and yearning as it called for its mate that we marvelled at the soul and intelligence which could make so stirring a song. When it finally ceased we slid down the steep hillside sitting on the crossed heels of our snow-shoes and using them like toboggans. Clouds of sparkling snow flew up behind us, and it was getting colder every minute.

After that night we heard the wolves on and off through March and part of April. Sometimes they sang alone or in choruses of perhaps ten or fifteen judging by the tracks found the next day. Often we started from our seats at the evening meal to hear wolves just across the lake from the cabin. These earlier choruses usually began with one or two voices, which were joined by the others, one by one taking up the song in perfect harmony. It was sheer music of the most thrilling heart-stirring kind. Later on in the spring, in April, when the mating season seemed to be over, we heard the hunting song. This was fierce and blood-curdling and eerie; it differed markedly from the love call. Our feelings of warm affection toward the wolves became tinged with respect and a slight chill.

During all this time we actually saw no wolves. The Indians who had lived in this country all their lives told us that they saw them very seldom in the forested parts, while on the big lakes and open plateau country they were a not uncommon sight. We knew, however, that the wolves saw us, for we found their great tracks where they had stepped aside from the trail while we went by. Often they followed us, no doubt from sheer curiosity. On one occasion when we were returning from a several days' journey, a broken snow-shoe harness caused one of us to drop behind the other in order to fix it. As we were about four miles from

LAKE TETANA. MARCH, 1938.

FISHING FOR TROUT IN DUG-OUT CANOE ALONG THE DRIFTWOOD RIVER.

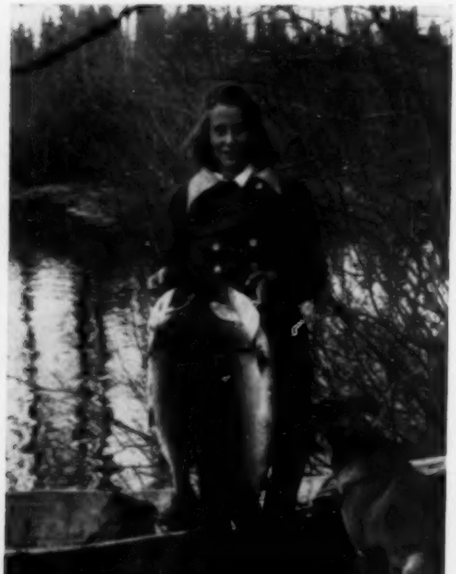
the cabin and night was advancing rapidly, the other went on ahead to break trail and get the fire going in the stove. The one behind was conscious of being followed to within a mile or so of the cabin, although she could see no visible signs of it and judged only by that intuitive sense which one soon develops after a few months in a country where there are no man-made sounds or signs of any sort, other than one's own. The next day two Indians, who had followed along part of our trail, asked us if we had seen the six wolves that had obviously followed us for some miles. These tracks were made at exactly the same time as the last snowshoe trail, they claimed. The Indians were usually accurate judges of the age of animal tracks and could often tell the hour at which a track had been made, just as they could tell the time of day within half an hour, without a timepiece.

In all that country we could learn of no first-hand cases of any human person having been attacked by wolves, although wolves were sometimes reported in companies of fifteen or even thirty individuals. That these northern timber wolves possessed a remarkably high order of intelligence was agreed upon by all authorities. The Indians around us had many superstitions regarding wolves; a rifle that killed a wolf would soon kill a human; or a person born at a certain time must never hunt or kill a wolf, because if he did, his nearest female relative was certain to become insane..

According to the game authorities the wolves in some districts were increasing to such an extent that they were becoming a serious menace to the moose population. On one of Sapolio's visits, he reported to us that he had just seen five moose that had been killed by wolves, all within a mile of each other. They had been hamstrung and only partly eaten. Later, however, the wolves returned and finished the carcasses. Wherever we found animals fed upon by wolves, there was no sign of waste, as there so often is when they are killed by the Indians or white man.

ALPINE MEADOWS, NORTHEAST OF BEAR LAKE, B. C. JULY, 1939.

In the autumn and spring, during periods of marked seasonal change, we



MRS. FLETCHER AND CATCH
DOLLY VARDEN TROUT WEIGHING 6¼ AND 8 POUNDS.

saw displays of the aurora borealis. These were much more vivid in the spring, when they appeared on and off for about one month, chiefly in March. They began around 10:00 P.M. nearly always over the heavy spruce forest to the east. They were yellow-green zig-zags which gradually became rose and lavender streamers reaching up to the zenith. To one unaccustomed to the awesome powers of the north the aurora as we saw it in March over our Driftwood Valley was one of the great marvels of life. Occasionally it took on an unusual form. One night at 1:00 A.M. we woke to see huge clouds of crimson rolling down the valley. The open patch of water on the lake and all the snow around reflected the deep red. And this in spite of a moon which was making the white world bright with light. Our first thought was of some big fire, till we realized that this was impossible in a world buried deep in snow with the nearest town of any size hundreds of miles away.

By the end of February our food supply, obtained in bulk the previous September, was in need of replenishing, and so early in March we went down to Takla Landing, sixty miles away, to do our shopping for the spring and summer. One of Bear Lake Charlie's sons followed behind us with four dogs and a toboggan, which carried our impedimenta and more specimens for the museum. We followed the tortuous Driftwood River most of the way, adding to our mileage, but making better time than would have been possible in the soft deep snow of the woods. By noon the sun's rays melted the crust which formed on the snow during the night. Our snow-shoes were wet and terribly heavy and the harness rubbed and blistered our toes badly in their wet socks and moccasins. The glare on the melting crust and the long hours of sunshine were tiring, and dark glasses essential. When we left the river to go through the forests, we traveled much more slowly. The trees were thick, the snow bad, and often at steep banks the toboggan crashed and slid downhill with the dogs tangled around trees and themselves. At noon on the second day we reached the north end of Takla Lake. The following morning we started off down the lake, our snow-shoes tied on the toboggan, for the ice on the lake was level and almost free from snow. At 11:00 A.M. we stopped near the shore to make tea, then on again, our eyes, even with the dark glasses, smarting and bloodshot from the intense glare on the glittering lake. After five and a half hours of travel we reached the settlement, having come twenty-five miles that day and approximately eighty miles altogether from our cabin.

The few white people at Takla Landing were most kind and hospitable and curious to see how we had survived the winter. But we were anxious to get back to the solitude of our cabin and Lake Tetana before two days had passed, for by that time we were more wedded to our wilderness life than ever.

During our year and a half in the Driftwood Valley region we had six

white visitors. One was an old gold prospector, with one hand, who stopped at our cabin on his way "out" in the spring. His Indian guide had deserted him when he had come half way to Takla, and his snow-shoes were badly broken. He had with him a beautiful Alsatian dog and her two pups, aged four months. They had already traveled on foot over one hundred and ninety miles. After two nights' sleep on our cabin floor and a good rest we saw him well on his way, with mended snow-shoes and minus one of the pups, "Rex," which he had given to us. This little fellow was soon to become our devoted companion and guard and was much admired—and respected—by the Indians.

In April, although the snow still lay deep everywhere, the winter silence over the land gave way to the rustle of wings and the noisy courting of birds. Ducks, geese and swans began to go northward and occasionally rested for a while on the partly open water of our small lake. The night of April 10th was warm and still with a misty rain and the moon showing faintly through the clouds. At 11:00 P.M., as we were just going off to sleep, we were roused by a deep melodious chorus of trumpets all around and over the cabin. We fell out of our beds and leaned from a window, to see four great white birds. They were low down, their bodies gleaming in the soft light as they circled over us. They were trumpeter swans (*Cygnus buccinator*), for no other species could make that music. Each voice was individual; the whole made an exquisite harmony. The four swans did not land, but flew off, and at the same time we heard more soft trumpets overhead and saw the tail-end of a V comprising twelve swans. Long after they disappeared into the northwest, we heard their fairy-like voices as they hailed each other along. On five other occasions, during April, we heard and saw more flocks of these majestic birds, all going northwest; we counted one hundred and ninety individuals. Some

WITH DOLLY VARDEN TROUT
MR. FLETCHER HOLDING 8½- AND 9½-POUND FISH.

passed us during the night, their musical toot-toots announcing their coming far ahead. One morning at sunrise, we stood in an open snow-covered meadow, as a flock of forty-three trumpeters passed slowly over us with the sun's rays on their wings and their lovely voices echoing up and down the valley in the still air.

After most of the trumpeters had gone north, the whistling swans (*Cygnus columbianus*) came through. Their notes were harsh by comparison, with something of a wheeze in them, and so distinct from the trumpeters that we felt there was little chance of confusing the two species. We counted two hundred ninety-three whistling swans. On November 1, 1938, on Bear Lake we saw fourteen swans, which were mostly immature, and permitted us to paddle within fifty yards of them. It was snowing hard at the time, and we were unable to photograph them. We saw a few other companies going south during the fall, but they were rather quiet and we could not distinguish the species. From In-

dians and others we learned that one of the possible reasons for the scarcity of trumpeter swans is the fact that at their winter quarters they are often caught by sudden frosts while sleeping, and are frozen into the thin ice, thus becoming easy prey for foxes and coyotes.

We had a fairly large variety of wild ducks on Lake Tetana throughout April and May. There were common mergansers (*Mergus merganser*), common and Barrow's golden-eyes (*Glaucionetta clangula* and *Glaucionetta islandica*), mallards (*Anas platyrhynchos*), green-winged teals (*Nettion carolinensis*), pintails (*Dafila acuta*), baldpates (*Mareca americana*), buffle-heads (*Charitonetta albeola*), shovellers (*Spatula clypeata*), ruddies (*Erismatura jamaicensis*), lesser scaups (*Nyroca affinis*) and one old squaw (*Clangula hiemalis*). The mergansers, pintails and teals were going through their mating activities in May in the rapidly enlarging open stretch of water below the cabin. The Barrow's golden-eyes were perhaps the most gorgeous of the ducks to look at. The males tossed their royal purple heads high above the velvet markings of their black and white bodies. Both sexes waved their heads from side to side, strutted and primped, and chased each other furiously, long into the night, with a clatter and whistle of wings. Two pairs were with us all spring and seemed unable to decide who belonged to whom. But they must have reached some agreement finally, for two females with their young were around our lake throughout the summer. On June 21st, one female with five youngsters, three or four inches long, was seen. We watched them on and off until early August. The male was very little in evidence, the mother apparently doing all the training and guarding of the young. We often saw her teaching them to follow close behind her in single file, or to float down the swift current of the river near the lake outlet without mishap. At the end of the summer two of the young had disappeared, but the others apparently grew up successfully.

LAKE TETANA. APRIL, 1938.

As the lake became more and more

open, large companies of ducks came in at night; sometimes we counted up to one hundred or more. Many of them would shoot down over our cabin just at dusk, at terrific speed, with a whirring of wings and a splash as they landed below us. At times they appeared to miss the cabin roof by a few feet only.

The spring days and nights were as full of sounds and active life as the long winter ones had been devoid of sound and live things. The noise of running water, winds amounting to gales, and singing birds, filled the days. Swallows, tree (*Iridoprocne bicolor*) and violet-green (*Tachycineta thalassima lepida*), exquisite in color, darted here and there over the lake. Ruby (*Regulus calendula calendula*) and golden-crowned kinglets (*Regulus satrapa olivaceus*) were everywhere. Rufus humming-birds (*Selasphorus rufus*), red-breasted sapsuckers (*Spyrapicus varius ruber*), eastern purple finches (*Carpodacus purpureus purpureus*), rusty and Brewer's blackbirds (*Euphagus carolinus* and *Euphagus cyanocephalus*), and various kinds of warblers: pileolated (*Wilsonia pusilla pileolata*), MacGillivray's (*Oporornis tolmiei*), redstarts (*Setophaga ruticilla*), magnolias (*Dendroica magnolia*), black-polls (*Dendroica striata*), Audubon's (*Dendroica auduboni*), etc., made the trees and shrubs around us gay with color. Olive-backed (*Hylocichla ustulata swainsoni*) thrushes around the lake shores and grey-cheeked (*Hylocochla minima aliciae*) along the river sang all the long evenings through, until darkness came.

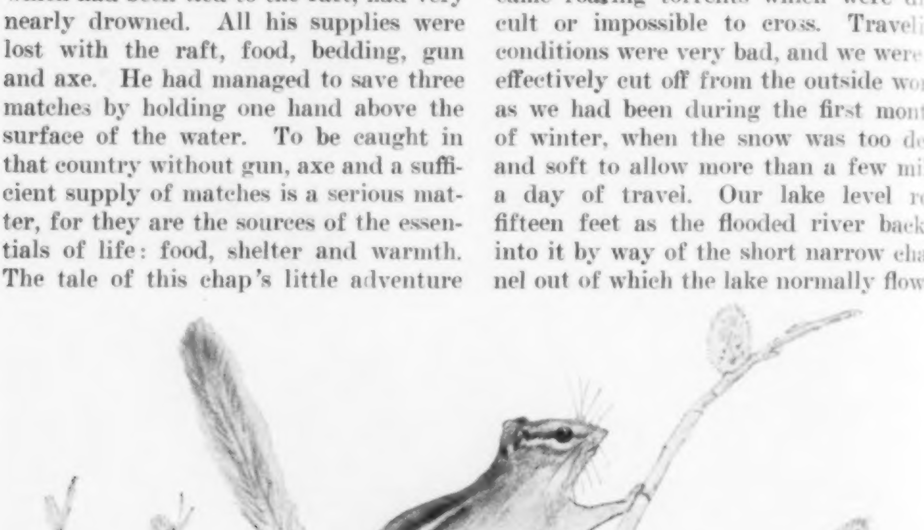
One May morning when we got up, we were surprised to find a strange Indian sitting on our wood-pile behind the cabin. With him was a dejected-looking dog. He was on his way from north of Bear Lake to Takla Landing, and had tried to come down the Driftwood River, which was in flood at that season, on a raft. The raft was wrecked

TOADS DURING MATING SEASON
MALE ON TOP OF FEMALE. MAY, 1938.

NEST OF MACGILLIVRAY'S WARBLER

at a log jam the night before, about a mile from our cabin, and he and the dog, which had been tied to the raft, had very nearly drowned. All his supplies were lost with the raft, food, bedding, gun and axe. He had managed to save three matches by holding one hand above the surface of the water. To be caught in that country without gun, axe and a sufficient supply of matches is a serious matter, for they are the sources of the essentials of life: food, shelter and warmth. The tale of this chap's little adventure came out by degrees in halting English, in the matter-of-fact way typical of the country. It was just one of those unnecessary things that was apt to happen, and there was no use making a fuss about it. He was lucky, he said, to have been so near to us and to have found our cabin, that was all. We fed him a breakfast of beans, bannock and coffee, for he had not eaten since early on the previous day, and his dog had a lump of meat that made his eyes bulge. We gave the man biscuits, meat, tea, sugar and matches to take with him. He made himself a kettle out of a tin can and wire, said, "Well—thanks" and departed, a damp, silent, philosophical figure.

From mid-April to the middle of June all the rivers and streams in the country were flooded. Innocent little creeks became roaring torrents which were difficult or impossible to cross. Traveling conditions were very bad, and we were as effectively cut off from the outside world as we had been during the first months of winter, when the snow was too deep and soft to allow more than a few miles a day of travel. Our lake level rose fifteen feet as the flooded river backed into it by way of the short narrow channel out of which the lake normally flowed into the river, and the crystal-clear green and blue depths turned muddy. Even the springs near the cabin were barely strong enough to push back the oncoming tide of murky water. By mid-June all snow was gone from the open country, though there were snow banks in the shaded parts of the woods. On the mountains it was still four to five feet deep.



GRAY-HEADED CHIPMUNK FEEDING ON PUSSY WILLOWS.
ON NUMEROUS OCCASIONS THIS SPECIES WAS SEEN TO FEED REGULARLY ON THE WILLOWS AS SHOWN HERE, DEVOURING AT LEAST 80 PER CENT.

In May we replenished our larder with trout from the river. Our casting rods did yeomon service, and many a rainbow (*Salmo gairdnerii kamloops*) and Dolly Varden (*Salvelinus malma*) gave us a double thrill—first in the catching, second in the eating. The snags and drifting logs of the swollen river made us

lose many fish and spoons. The Dolly Varden were the most common, and we often caught them weighing eight to ten pounds. When we cleaned the fish we found eggs or milt in most of them, indicating that this was their spawning season in our part of the world. Later, we obtained lake trout (*Cristivomer namaycush*) from Bear Lake, but these were sluggish and by no means as fine eating as the trout from the Driftwood River.

When the floods subsided the lake became clear once more. The flowers bloomed everywhere, and the bears hunted for grubs in dead and rotten logs. Our lake seemed suddenly to be alive with swimming toads (*Bufo boreas boreas*) and warm evenings echoed with their shrill and not unmusical trills. When we went out in our dug-out canoe, recently purchased from Bear Lake Charlie, we picked up toads, single and in pairs, which were swimming as much as two hundred yards from the shores. The females, larger and much more vivid in color, were a bright reddish brown or green with cream markings. The males rode on the backs of the females, their thumbs clasped beneath the latter's front legs in what was practically a death grip, in which position they would remain until the eggs were extruded and fertilized by the male sperms. We collected five or six mating pairs, as well as some single individuals, and put them in jars and pots inside the cabin. Even when caught and put in new quarters the males did not release their grip of the females. After four days one female extruded her eggs in a long, tangled string of jelly. We hunted extensively, but could find no eggs in our lake, nor did we ever discover any tadpoles during the entire summer, though we saw millions in some of the much warmer lakes and ponds near us. What happened to the eggs of the mating toads of our lake, if they left their eggs there at all, is a mystery still to be solved. During the summer and two autumns which we spent in this Driftwood Valley region, we found the adults of this common toad everywhere on land. On the other hand, we found only two species of frogs (*Rana pretiosa* and *Rana cantabrigensis*), and these were rare. We saw two of the former and three of the latter. In all that time we had also seen only one salamander (*Ambystoma macrodactylum*), and this had been found near the lake shore by one of the Indians who helped us to build the cabin. It was five inches long, black on the top with a marbling of greenish-gold markings, mottled with gray on the belly.

At the time when we collected the mating toads, we showed the pairs to two of Charlie's sons, explaining their methods of "making children," as the Indians termed it. To our surprise the boys jumped away and hurried out of the cabin. We later learned that some of their most serious superstitions were connected with "frogs." It was quite taboo for them to touch or get near "frogs." Such an action meant death or illness and all sorts of horrible things. There were superstitions regarding the salamander also, which "carried gold on its back," and to touch one might mean that a man would lose all his worldly possessions. The Indians usually told us of these beliefs with slight grins, but at the same time they did not want to take any chances—"in case."

"THE APOTHEOSIS OF SCIENCE"[1]

By ROBERT LOVELL BLACK

EMMERICH MANUAL TRAINING SCHOOL, INDIANAPOLIS, IND.

"The Apotheosis of Science," a mural painting of heroic conception, was executed by Elmer E. Taflinger, of Indianapolis, Indiana. For five years Robert Lovell Black and Mr. Taflinger collaborated, as scientist and artist, in producing an artistic, historical and educational canvas in honor of science and the great scientists who have created it. The completed painting is now installed for the inspiration of students in Mr. Black's classroom at Emmerich Manual Training High School, Indianapolis.

The painting is twenty-one feet long and five and one half feet high, in oil, on canvas, and is divided into three panels. The center panel (Fig. 1) portrays representative plants and animals of each of the main classes of each phylum of the animal kingdom and plant kingdom. The center portion of the center panel contains four Greek scientists of the ancient world, two on either side, each symbolical of one of the four ancient elements, fire, water, earth and air. They are placed in defensive positions, guarding life as

[1] "A History of Science in Mural Painting. Fifty-two Scientists and a Classification of Living Things." Robert Lovell Black, Emmerich Manual Training High School, Indianapolis, Indiana.

FIG. 1. CENTER PANEL

FIG. 2. LEFT PANEL

shown in the center. In the side panels are shown forty-eight of the great scientists of all ages, painted in naturalistic positions, and arranged according to their field of work rather than the period in which they lived. The men in the left panel (Fig. 2) were interested in living things, mainly in the fields of botany, zoology, physiology, anatomy, genetics and medicine; while those on the right panel (Fig. 3) dealt mainly with pure science as represented by mathematics, physics, astronomy and chemistry. Each scientist has reached a summit in some realm of scientific achievement and is portrayed as standing on a mountain top with valleys and peaks in the background. Each one holds in his hand a symbol of his accomplishment.

The foreground of the picture shows the remains of great periods of civilization, which have risen and fallen while the search for truth by the scientists has continued steadily forward. Such objects as the helmet of Menelaus, Alexander's sword, a standard bearing the S.P.Q.R. of the Romans, the torso of Hercules on an Ionic column, a Roman millstone and arch, an astrolabe, a globe, the unfinished statue of the slave by Michelangelo on top of a Renaissance column, manuscripts, a hat of Napoleon, the skull of his horse with a sword thrust through it, skulls of soldiers, remains of architecture, wrecked trees and timbers, twisted steel beams, a broken air-plane propeller, a helmet of a soldier in the World War, and a standard of the A.E.F. symbolize

the ancient world, the Middle Ages, the Renaissance, the Napoleonic period and the World War.

One of the most interesting things about the production of the painting was the way in which the fifty-two scientists were chosen. A list of twenty great scientists of all periods was submitted to more than a hundred scientists, editors, university professors and prominent people in various walks of life. They voted on the names sent to them, and in many cases sent back additional names which they thought should be included. These answers were analyzed and tabulated. As a result, two names, Pliny and Abelard, were eliminated from the original list, and thirty-four new names were added. Among the scientists consulted were Robert A. Millikan, Arthur H. Compton, Karl T. Compton, Thomas Hunt Morgan, William Hornaday, William Beebe, Morris Fishbein, Otis W. Caldwell and Tenney L. Davis.

The names of the scientists who were selected are in order from left to right as they appear in the painting. In the left panel: Lyell, Humboldt, Audubon, Lamarck, Linnaeus, Helmholtz, Cuvier, Roentgen, Vesalius, Harvey, Leeuwenhoek, Langley, Schwann, Bernard, T. H. Morgan, Schultz, Darwin, Mendel, Lister, Pasteur, Galen and Alhazen. In the middle panel: Aristotle, Hippocrates, Archimedes and Euclid. In the right panel: Roger Bacon, Copernicus, Watt, Galileo, Newton, Huygens, Laplace, Descartes, Einstein, Leibnitz, Planck, Mendelejeff, Franklin, Lavoisier, Galton, Faraday, Rutherford, Arthur Compton, J. J.

FIG. 3. RIGHT PANEL

Thomson, Millikan, Maxwell, Hertz and Crooks.

The center panel, which shows the classification of plants and animals, presented a complex problem which was worked out on a plan of concentric circles. The composition of the painting, the colors and the lighting all work together to simplify and unify the subject-matter. In three small concentric circles in the lower portion of the center panel appear representatives of each of the classes of the nine major phyla of the invertebrates. In the middle circle appear Protozoa, Porifera and Coelenterata, a cell, complete in detail, and various stages of oogenesis, spermatogenesis and embryology. In the circle on the left appear Platyhelminthes, Nemathelminthes, and Echinodermata. In the circle on the right appear Annelida, Mollusca and Arthropoda.

In the large circle in the center of the panel are representatives of each of the sub-phyla of the phylum Chordata. The representatives of each of the six classes of sub-phylum Vertebrata, Elasmobranchii, Pisces, Amphibia, Reptilia, Aves and Mammalia occupy most of the area of this large circle. In the center of this circle is a semi-transparent three-headed, four-armed, single-trunked, four-legged symbolical figure which represents the three primary races of the family Hominidae. The head in the middle represents the Caucasian race. It is a copy of the self-portrait of Leonardo da Vinci, as typifying the highest "all-round" man, painter, sculptor, architect, musician, engineer and scientist. The heads to the left and right behind da Vinci represent the Negroid and Mongoloid races, respectively. The design of this part of the painting was based on da Vinci's study of Vetruvius. He found that when a man stands with feet together and arms spread horizontally, the figure forms a square, and that when a man stands with feet spread apart and the arms raised to a forty-five degree angle (or "spread-eagle"), the figure forms a circle. Ingeniously worked into the background of the large circle are representatives of the classes of the four major phyla of plants, Thallophyta, Bryophyta, Pteridophyta and Spermatophyta.

COOPERATION IN ASTRONOMY

By Dr. OTTO STRUVE

DIRECTOR OF THE YERKES AND McDONALD OBSERVATORIES

THE McDonald Observatory of the University of Texas was officially dedicated on May 5, 1939. It is equipped with a reflecting telescope 82 inches in aperture, and it is, by virtue of this fine equipment, one of the important centers of astronomical research of the world. The observatory is operated jointly by the University of Chicago, which supplies the staff from its own Yerkes Observatory at Williams Bay, Wisconsin, and provides the larger part of the operating funds, and by the University of Texas, which built the entire plant from the bequest of W. J. McDonald, of Paris, Texas, and which, in addition, pays for part of the maintenance.

The plan of collaboration, entered into by the two universities, has been very successful. Without this plan, the 82-inch telescope would not have come into existence; the Yerkes Observatory would not have been able to increase its scientific output and would not have attracted to its staff new astronomers of high international standing; the University of Texas would not now own one of the world's most powerful telescopes, and would not have become the sponsor of some of the most intensive research in astronomy.

Although the new telescope has been in operation for only nine months, some interesting scientific results have been secured with it. Dr. Polydore Swings, visiting professor at the University of Chicago, has already announced the discovery of a large number of "forbidden" radiations, hitherto unknown, in stellar spectra photographed by him last month at the McDonald Observatory. Forbidden radiations can not be observed in the physical laboratory, and without astronomical observation they would have remained unknown. Although these radiations are "forbidden" on the earth, they occur in the vast spaces of the universe where low density of gas, almost limitless space and permanent semi-darkness are conditions required for their origin. It is unnecessary to dwell upon the importance of this knowledge: the physicist knows that to understand the properties of matter he must understand the radiations which matter is capable of producing. He has studied in the laboratory the "permitted" radiations, but his data would be unintelligible without a knowledge of the "forbidden" radiations.

The most important discovery made by Dr. Swings is that of a forbidden line of the element Fe X (iron ten). Under normal conditions the atom of iron possesses twenty-six electrons. When energy in the form of light or in the form of a shock—perhaps a collision with another particle—is applied to an iron atom, it may lose its outermost electron, after which it would be designated as Fe II. If more energy is applied, it may lose two electrons and it then becomes Fe III, and so on. The energies required to produce these changes are great. In the atmosphere of the sun we observe Fe I, with a little admixture of Fe II, and with no Fe III at all. Little imagination is required to realize the tremendous amount of heat and light in the surface layers of the sun. And yet, they are barely sufficient to create and maintain Fe II in a gas whose density is ten thousand times less than that of the air we breathe. In some of the hottest stars Fe III was found last year by Dr. Swings,

and was confirmed through a study of photographs made with the 82-inch reflector.

Astrophysicists have established a scale of energies to measure the forces which must be applied to an atom to break off its various electrons. The unit of this scale is the electron-volt that is set, by agreement, equal to the energy which is acquired by an electron when it passes through a potential difference of one volt. To knock off the first electron of an iron atom requires 8 electron-volts; to produce Fe III requires 16 electron-volts. But to produce Fe X requires about 200 electron-volts—an energy which has not hitherto been contemplated in astrophysical studies! This is in itself remarkable, but to the physicist a still more significant point is the fact that the forbidden line is produced by a transition between the sub-levels of the same multiple state. No other forbidden line of this type had ever before been observed, although Dr. Theodore Dunham at Mount Wilson had inferred, theoretically, that such transitions must occur in the gases which fill the spaces between the stars.

Dr. Gerard P. Kuiper, of the Yerkes Observatory staff, is now engaged in the discovery and study of some very strange stars which we designate "white dwarfs." This name gives but an imperfect idea of the remarkable properties of these stars. They are called "dwarfs" because they are small—some are smaller even than the earth. And they are "white" because their temperatures are high—of the order of 10,000° Centigrade at their surfaces. What the name does not indicate, and what is really of most importance, is the fact that in mass they are not dwarfs, for their masses are of the order of that of the sun. If you could squeeze a mass comparable to that of the sun into a volume smaller than that of the earth, you would have a density so enormous that it defies the imagination. Kuiper has computed for one of his stars a mass of one thousand tons per cubic inch. Such matter is not ordinary matter at all—we have nothing like it on the earth or on the sun, and we call it "degenerate matter." The problem is to find, by means of astronomical observations, the properties of this "degenerate matter." We already know that it follows quite different laws of physics than does ordinary terrestrial matter, and we hope that we shall ultimately discover how it reacts to changes in pressure and temperature.

For years astronomers have been interested in the problem of the chemical composition of the stars. The question is an intricate one. The tremendous energies radiated by the stars into space in the form of heat and light are almost certainly produced by the slow conversion of certain kinds of atoms into other forms. We have heard much in recent months of the so-called carbon cycle, of the proton-proton mechanism, and of other processes which may account for the heat and light of the stars. These processes gradually use up the available hydrogen and thereby reduce its abundance. There is ample indirect evidence that the hydrogen contents of the stellar interiors are not all the same. The only way we can directly observe the composition of a star is to study its spectrum.

It might appear to be an easy task to determine the amount of each chemical element in the outer layers of a star from its spectrum. In reality, the problem is very difficult, and little trustworthy information has thus far been obtained. In a few cases we suspect that hydrogen is less abundant than it is in normal stellar atmospheres, but the evidence is limited to some rather unusual objects whose spectra are quite difficult to interpret. At the McDonald Observatory, Dr. Jesse Greenstein observed the star υ Sagittarii, which had been previously observed at the Yerkes Observatory and elsewhere, but only in the blue and violet

regions of the spectrum. The new work, carried out in ultra-violet light, was facilitated by the high reflecting power of aluminum with which the 82-inch mirror is coated and by the excellent transparency for ultra-violet light of the quartz prisms of the spectrograph.

The work on υ Sagittarii shows that it is a star whose atmosphere contains an abnormally small amount of hydrogen. From the point of view of chemical composition, it is by far the most interesting object in the sky, and it is very remarkable in another respect: In normal stars of its class, which have surface temperatures of about 10,000° Centigrade, the atoms of hydrogen and the free electrons produce a certain amount of haziness in their atmospheres which prevents us from seeing into their deeper layers. In υ Sagittarii, however, the hydrogen content is low, the metals contribute a relatively greater share to the free electrons, and the opacity, or haziness, of the atmosphere is, therefore, not the same as in a normal star. The theory of the formation of stellar spectra depends entirely upon this opacity. There has previously been little opportunity to secure observational evidence in support of this conclusion, but υ Sagittarii provides the ideal material for testing the theory.

Since its installation on Mount Locke the 82-inch reflector has been in constant demand and has provided a continuous flow of scientific material. Dr. A. Unsöld has collaborated with me in studying the ultra-violet spectra of B and A stars; Unsöld has also determined the curve of growth of Arcturus; Roach has cleared up some of the mysteries of P Cygni; and Page has observed the spectra of planetary nebulae. A program of radial velocities of faint B stars is being carried out by Popper and Seyfert.

All this work owes its inception to the cooperative arrangement between the University of Texas and the University of Chicago. Astronomers have always been eager to cooperate. They have carried out large international projects, such as the complete mapping of the sky with photographic telescopes distributed all over the world, and the determination of accurate star positions with meridian circles in a dozen or more observatories. They have organized international bureaus for the distribution of astronomical news, such as discoveries of comets and novae. But universities have rarely been willing to pool their resources for the maintenance of a large observatory. The Chicago-Texas arrangement, and the somewhat similar arrangement between Ohio State University and Ohio Wesleyan University in maintaining the Perkins Observatory, are a new demonstration that satisfactory results can be obtained by collaboration.

The greater number of existing astronomical observatories in the United States were built and equipped from funds donated by private persons. Most of these institutions were opened when instrumental equipment was not as highly developed and not nearly as costly as it is to-day. When the plans for the Yerkes Observatory were first considered in 1892, the secretary of the university recalled that, not many years before, the old University of Chicago had owned the largest astronomical instrument then in existence—a telescope (now at the Dearborn Observatory) having an objective 18½ inches in diameter. Since that time telescopes had been made with objectives having diameters of 20, 23, 24, 25, 26, 27, 28, 30 and 36 inches. The Yerkes 40-inch refractor was completed in 1897; but a few years later the 60-inch reflector at Mount Wilson and, finally, the 100-inch Hooker reflector at Mount Wilson became, in turn, the world's largest telescopes.

Large apertures mean great light-gathering power. Not all astronomical investigations require great light-gathering power, but many do. An astronomer

who has at his disposal a 15-inch telescope can carry on certain types of research, but he can not participate in some of the most interesting and fruitful investigations now in progress at the larger observatories. Doubtless many able astronomers have experienced a deep feeling of disappointment when limitations of instrumental facilities have prevented them from carrying on the type of research which they considered most useful and valuable. Yet, restrictions of university budgets and the rapid decrease in the frequency of large private donations leave little hope for the improvement of small and inadequately equipped observatories. During the period of 31 years between the construction of the Dearborn 18½-inch telescope and the Yerkes 40-inch telescope, the priority in aperture passed through ten different institutions. Since the completion of the Mount Wilson 60-inch reflector in 1908 it has remained with the same institution.

There can be no doubt that the growing disparity between the facilities for research available at various observatories raises a serious problem for the future development of astronomy. Fifty years ago practical work in astronomy was rather uniformly distributed among a large number of observatories in Europe and America. As time goes on we see a growing tendency to reduce observational activities at the smaller institutions. The existing equipment is usually insufficient and funds are not available for a complete modernization. Moreover, at some of the eastern and middle-western institutions the climate is not good enough for the efficient utilization of a large telescope. The organization and maintenance of a separate observing station in the southern hemisphere, or even in our own Southwest, is beyond the resources of the average university.

Hence, there has been a general tendency to substitute theoretical studies for observational work. Fortunately, in the United States this process has only started. But in Europe we have seen the gradual decline of observational work and the rise of theoretical institutions. The Kapteyn Institute at Groningen, Holland, the theoretical astrophysical institute at Oslo, Norway, the substitution of a theoretical department for what was once the University Observatory at Kiel, Germany, and, above all, the rise of theoretical astrophysics in England during and after the World War are striking examples.

I believe we must guard against this tendency in the United States. After all, the success of theoretical study depends essentially upon the supply of observational results. These now come almost entirely from a few large American observatories. There is, of course, no danger that Mount Wilson, Lick, Harvard and a few other observatories will not continue their observational activities on their present scale. But is it wise to restrict all observational work to a small number of institutions? Is this not likely to produce a cleavage between theoretical astronomers and observers which will result in much confusion and unsatisfactory progress? Is it not also rather disquieting when we contemplate the number of young astronomers who are being trained for future careers at institutions which are unable to carry on modern observational activities? The present trend toward pure theory is not a natural process, but is one which is forced upon astronomy by restricting circumstances.

I fear that unless something is done toward equalizing the research opportunities of all astronomers there will be a gradual deterioration of many observatories which, in the past, have been able to carry on investigations of a quality comparable to that of the largest institutions. Half-measures are expensive

and are not satisfactory. The difficulty which is normally experienced in a present-day observatory of limited means is not a lack of problems which can be attacked and solved with the available equipment. Even the most modest observatory can do *some* useful work. Important contributions to science are often made with small equipment. Ross's Milky Way Atlas was made with a 5-inch lens, Merrill's catalogue of emission stars is based largely upon observations obtained with a 10-inch telescope and the Harvard spectroscopic surveys were made with instruments of relatively small aperture. But it is significant that all these investigations were made at large observatories, in spite of the fact that such instrumental equipment could have been easily available at a small observatory.

The difficulty is, of course, that the small observatory is compelled to search for something that it is able to do, instead of doing what is scientifically important and interesting. An observatory which has only a 12-inch visual refractor can do useful work on double stars and variable stars, but the limitation is quite likely to destroy initiative even in this restricted field. When a scientist is unable to do what he considers necessary and is constrained to do what his telescope allows him to undertake, he not only loses interest but often loses his contact with modern developments; his department deteriorates; his students suffer from the narrowness of the institution's interests. In the meantime the observatory continues to draw heavily from the university's general budget. With normal deterioration the instruments and buildings are more than likely to require increased appropriations as time goes on, but the scientific output becomes, if not less in volume, certainly less in value.

Fortunately, it is not necessary for this process to continue. A powerful modern telescope provides a wealth of material. Some of it can be utilized immediately by the observer. But more often than not the photographic plates contain valuable material which is not immediately used and which is often not even appraised by the observer. It is certain that by cooperation we can do our telescopic work more efficiently than we have done it in the past, and supply a greater number of astronomers with material.

If several observatories would pool their resources they would be able to construct a series of instruments designed for special purposes. The whole equipment would constitute a first-class observatory, although separately each instrument might not be of great value. For example, the McDonald Observatory has a large reflector which is suitable for work with slit spectrographs. But it has no powerful camera for objective-prism work and may never be able to purchase such an instrument. It would be advantageous for us to collaborate with some other institution which may be contemplating the construction of a new instrument. Without collaboration each observatory builds what appears to be now the most generally useful instrument—a parabolic reflector—even though there is already a considerable amount of duplication in reflectors of moderate size.

The resources of the McDonald and the Yerkes Observatories are sufficient for successful work in many phases of astrophysics. They are not sufficient for the development of new fields of research, such as solar physics, objective-prism spectroscopy, the study of cosmic rays or of cosmic radio disturbances. Moreover, the 82-inch telescope is not particularly suitable for certain types of astrophysical research and for which more efficient types of instruments have been designed. In order to enlarge the scope of the work of the McDonald Ob-

servatory it would be reasonable to invite the collaboration of other institutions.

Let us suppose that a plan of collaboration could be worked out which would be satisfactory to all participating institutions. We should then be able to organize jointly an observing station in the Texas mountains, where the McDonald Observatory is located, which would be much more powerful than the present McDonald Observatory alone. The participating institutions would all profit from the fine climate, which yields nearly 300 clear or partly clear nights each year; from the excellent seeing which averages much better than in the Middle West; from the exquisite transparency of the air at an altitude of almost seven thousand feet; and last, but not least, from the latitude of N 30° which permits the observation of a large part of the southern sky. The joint enterprise would benefit from the participation of many competent astronomers whose services no one observatory could possibly afford. Duplication of effort, no less than of telescopes, would be avoided. Many special types of research which require a large amount of preliminary laboratory work—for example, radio-metric measurements, photoelectric photometry and various other applications of electrical methods—could be prepared at the participating institutions and could be divided among them. Altogether, it would seem to be conservative to say that the plan would increase the observational facilities which are now available to many astronomers and would, thereby, make their present connections more attractive. It is my strong conviction that astronomers and university administrators should seriously consider such a project.

The plan would, of course, be expensive. But it would cost much less than equipment for separate new observatories. It would leave the present organizations intact and would secure for each participant a new outlet for research. The local observatories could continue their present functions and could, in addition, serve as laboratories for the measurement and discussion of material obtained at the observing station. The astronomers would from time to time travel to the observing station in order to gather material. They could be available for teaching at all other times and could conduct most of their scientific work in their offices at home. The constant pressure for new telescopes on the various campuses would be materially relieved. Measuring machines, photometers, etc., that would still be needed by each institution, would be relatively inexpensive.

Although the suggested plan for wider cooperation among astronomers presents some difficulties, they are probably no more serious than those that have already been overcome in carrying out earlier national and international undertakings. But, however serious they may be, they should be resolutely met, both because the scientific results to be obtained promise to be very important and because in putting such a plan into effect astronomers will set another notable example of cooperation.

WHY WE EAT WHAT WE EAT

By WARREN T. VAUGHAN, M.D.
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WHY do we eat what we eat? Possibly this should be preceded by another question, "Why do we eat at all?" The answer is elementary: we eat because we have to, because we are hungry. Also we eat because we like the taste of things. Many a fat old dowager eats chocolate peppermints, not because she is hungry but because she likes chocolate peppermints. As a corollary, we also eat because we are in the habit of eating. In this country we are in the habit of eating three meals a day. The Britisher is not actually a different species of animal that requires four meals daily, but he has found tea and crumpets in the afternoon a pleasant custom and has made it habitual.

The same three premises will be found to apply also to the original question. What we eat depends in part upon necessity, in part on habit and in great measure on taste. Convenience is also a factor. The newborn babe does not suckle at his mother's breast primarily because he or his mother knows that mother's milk contains most nearly the ideal proportions of protein, fat, carbohydrate and minerals. He does so in part because it is the most convenient thing to do. Mother's milk is not an absolutely necessary food. Many children are raised, from birth, on cow's milk, and some who are allergic thereto thrive on substitute food mixtures which contain no milk of any sort.

Down through the ages, from the earliest savages, dietary habits have been conditioned in great measure by the availability and convenience of the various foods, their palatability and by past experience with them on the trial-and-error basis. Experience has taught us concerning their taste, nutritional value and harmlessness.

As we sit in one of the more sumptuous restaurants in a large city and glance over the many pages of the à la carte menu we might wonder to what kind fate we owe our opportunity to order any number of the most delectable concoctions garnered from the farthest corners of the earth. Aladdin could not have done as well, since many of the finest of these foods were unknown to him in his remote time. We no longer stroke the lamp, but with a few strokes of the pencil we are far better off than he was. To-day nearly all of the really good foods on earth are available nearly everywhere, convenient as the corner grocery, palatable as man and nature can render them and guaranteed reasonably harmless by food laws and inspection. A good family dinner of to-day would render a Roman emperor of the banquet era green with envy.

How has this been accomplished? Several years ago an interesting novel started with the collapse of a bridge, the Bridge of San Luis Rey. On the bridge at the moment of the catastrophe there were a number of persons, some of them total strangers. The remainder of the book traced the former life of each of the victims up to the moment of the collapse, thus bringing to light those forces which gradually brought these victims together for their final destruction. As we sit, ready to destroy the delicacies before us, it would be interesting to trace them, likewise, back to their original sources. Space will not permit discussion of too large a number of our victims, but those

selected will serve as examples for the experiences of others.

Our story must start with earliest times, when more or less isolated groups of the human race were scattered here and there over the earth, and before the words trade and commerce had been invented. Shall we start with the Garden of Eden near the eastern end of the Mediterranean or shall we be more modern and commence on the plateaus of Tibet? Shall we be ultra-modern and assume that those precursors which ultimately became man might, like plants, have started at several places on the earth, provided conditions were right? It makes little difference in the present discussion, although in passing we might point out a fallacy in the story of Eden. Tradition to-day has it that the apple was the cause of the downfall. Botanists tell us, however, that this fruit had its origin in cooler climates, northern Europe and especially northern Asia. The apricot appears to have been the more likely contender for the honor, since it appears to be indigenous to Asia Minor. One might argue that if the apple story is true, the Garden of Eden was not in Asia Minor but more nearly at the site now more widely accepted as the cradle of the human race. Parenthetically, however, the Bible makes no mention of an apple. It merely alludes to the fruit of the tree of knowledge.

We might use the apple as an example of the method of propagation and distribution of foods. It seems improbable that all varieties of apple came from a single ancestral tree. To-day there are thousands of varieties within this family, *malus*. Some are edible, while others are not. It seems probable that, under proper conditions, plants closely resembling each other and now all grouped within the apple family took their origins independently, in the same way that the wheat of to-day was derived from the wild grasses of Asia Minor, while Indian corn was developing entirely independently from the teosinte grass of Mexico or from another local ancestral grass.

The crab apple of North America is indigenous to the New World and presumably was developing independently while the finer edible apples were evolving in Eurasia. But the point to be made is that those varieties which came to be used as foods usually took their origins from some unusually fortuitous specimen and have been distributed across the continents from this original source. To-day North America is the greatest apple region in the world. We have our indigenous members of the family, most of which are still wild and scarcely edible, but the cultivated apple of North America was originally imported into this country from Europe and more remotely from its original habitat in the cooler climates of the Old World. To be sure, man has improved the fruit by fertilization, selection and cross-breeding, until there are now hundreds of more delicious varieties descended from the original parent.

But the happy fact is that most of those foods cultivated for use by man may be traced back through historical records to an approximate original source, even though there are inferior domestic varieties which are probably indigenous to particular areas.

It makes a rather thrilling picture to visualize nomadic tribes wandering here and there within rather restricted areas; coming by accident upon an unusually delectable specimen of a plant which they have been accustomed to use as food; returning to the same plant whenever feasible, to again enjoy its delicious morsels; and then, as they become less nomadic, taking seeds or cuttings from this particular specimen, to plant in a more convenient place nearer home; nurturing it most carefully, protecting it from the weather and feeding it as it grows, thus establishing the earliest rudiments of husbandry. Into the sequence of the picture,

next comes contact, either peaceful or warlike, with other more or less remote tribes; realization that others have likewise developed better specimens of different foods; and the resulting exchanges by barter or by importation following conquest, this being the first step in the spread of cultivated foods across the earth.

Much of this occurred in prehistoric time. Carbonized apples have been found in the habitations of the prehistoric Swiss lake dwellers. It is true that these may have been the original wild apples rather than cultivated varieties. Apples were known to the ancient Romans and Phoenicians, who raised them in their gardens.

Before the dawn of written history man made a great discovery which enabled him to depart from that nomadism which forced him to change his abode with the seasons, so that he might always be where food was available. The discovery enabled him to remain permanently in one place. This was the cultivation of wheat and the making of flour which could be baked into bread for use when fresh vegetables and game were not available.

The origin of wheat is not definitely known, but it appears to have been developed originally from the wild grasses of Asia Minor or Egypt or around the shores of the Caspian Sea. It was introduced into China about 3000 B.C. and was described as being present in Egypt about 2440 B.C. It was used by the Swiss lake dwellers. Fortunately, other groups had also learned to cultivate grasses indigenous to their own territories for use as food. Rye is supposed to have originated in the Orient. It has been cultivated by man probably as long as has wheat. Both were used in the Bronze Age. However, rye was not cultivated in ancient India, Egypt or Greece. It is to-day the principal cereal of northern Russia, Scandinavia and northern Germany.

Barley was probably the first crop grain of the human race. It was described in Egypt as early as wheat, and the Egyptians claimed it to be the first of the cereals used by man, introduced by their goddess, Isis. It was a sacred grain to the early Greeks, used in sacrifices and in the cereal festivals. Pliny called it the most ancient cereal. The Cimbri, early progenitors of the Britons, made their bread from barley, which remained the chief food grain of England until as recently as the eighteenth century.

Rice is the most extensively cultivated of the grains and is the principal cereal food for over one third of the entire population of the earth. It appears to have originated in tropical Asia and was introduced into China about 3000 B.C. The ancient Romans knew the grain, but it was not introduced into cultivation in Europe until the sixteenth century.

Corn appears to be indigenous to the region of Mexico. It has been in cultivation since prehistoric times and is unknown in the wild state. Columbus first saw corn in Cuba in 1492. He carried it to Spain, from where it was rapidly distributed to most of the regions of the earth. When the new world was discovered corn was in cultivation from Canada to Brazil and from California to Chili. Some of the Icelandic sagas described as early as 1002 A.D., what may well have been corn on the New England coast. The early explorers following Columbus described the cultivation of corn and lima beans, along with pumpkins, by the Indians in the New England region.

As tribes grew larger and, for economic reasons and purposes of protection, banded together into nations, the distribution of cultivated foods within the nations was facilitated and commerce between them developed. Now, perhaps, we are in the era of the caravan routes across Asia, when trade dealt not only with hides and cloths, precious metals and jewels but also with the less highly perishable of the foods from foreign lands. Chang Chien, Chinese explorer, had established

overland trade routes between China and the Roman Empire by 115 B.C. As the routes of travel, by land and by sea, reached farther and farther, the spices eventually made their appearance in the Mediterranean countries. They were not quickly perishable, and they stimulated the palates of the Europeans as nothing had done before. Almost from the day of their appearance, exploration and commerce were guided in great measure by the desire of the white man for spices and more spices. This desire was a potent factor in Columbus' discovery of America, Magellan's circumnavigation of the globe, and the early settlements in America under the British East India Company. Love of spices was the cause for many a war. Attila, the Hun, required three thousand pounds of pepper as a part of the ransom of Rome. Many were the massacres countenanced in the Dutch East Indies in an effort to retain a monopoly on spices.

As the various peoples learned of the uses of their own foods, and their value in commerce, they often made every effort to establish monopolies. On many occasions attempts were made, sometimes successfully, to steal the secrets. An outstanding example occurred, not in horticulture but in sericulture. The Chinese had preserved the secret of silk manufacture for many centuries. In 552 A.D. two monks who had lived for some time in China first smuggled silk worms, in a hollow bamboo, to Constantinople, where, under the protection of the Emperor Justinian, they inaugurated the silk industry in Europe.

Coffee was indigenous to Abyssinia, where the natives ate the raw grain as a stimulant. In the fifteenth century the Arabs discovered the value of the bean and started its cultivation in southern Arabia. From the port of Mocha, knowledge of it spread to Egypt and Constantinople in the sixteenth century, to Venice and then to England in the seventeenth century. It was then that coffee houses and cafés sprang up in the European centers. Religious zealots denounced coffee as an intoxicating, insidiously pernicious drink. The Arabs kept their secret until the eighteenth century, when coffee was grown successfully in Java. To-day Brazil is the world's greatest coffee-producing country.

Chocolate first became known to the white man when Montezuma, the Aztec Emperor, gave Cortez a drink of the delicious beverage from a golden cup. The Spaniards carried cocoa back to Spain, keeping its source secret for many years, selling it at a high price, as chocolate, to the wealthy classes in Europe.

Cinnamon, native of Ceylon, was known to the ancient Hebrews, Greeks and Romans, but was not cultivated by them. It was carried across Asia Minor by the Arabs, who kept its source secret for nearly one thousand years.

Apricot is native of Armenia, Arabia and the upper portions of Central Asia. The fruit was held in such high esteem that, according to Disraeli, Tradescant joined a crusade against Morocco in 1620 for the sole purpose of stealing apricots for import into Britain. The cultivation of apricots in England dates from that time.

In one way or another, we see, then, that foods relished by one group of persons were gradually disseminated to other parts of the world. At times the route was quite circuitous, as in the case of the Irish potato. This food, native of the mountainous regions of Chili and Peru, was unknown in the hotter climate of Mexico, at the time of the discovery of America. From South America it was carried to southern Europe, whence it made its way to Ireland. It was later introduced into New England by a group of Irish colonists. Here was a New World plant, introduced into a different part of the New World via the Old World. In this way it succeeded in passing the

barrier of the tropics, where it does not grow. Its cousin, the tomato, made easier progress northward from South America, since the barrier did not prevent its propagation. Early explorers found the edible varieties in wide use in Mexico, as well as South America, and according to Jefferson it was being grown in Virginia in 1781. It was not, however, until after 1812 that the tomato came into use as a food in this country. The prejudice against it was probably due to two factors. Tomatoes were supposed to be poisonous, possibly because of their relationship to the deadly nightshade. Also, the earlier tomatoes which had not been intensively cultivated were by no means as good as they are to-day.

The foods which we eat to-day may be fairly accurately traced back to nearly all parts of the world. To Asia we are indebted for tea, rye, onion, rhubarb, buckwheat, radish, pistachio, licorice, peach, cucumber, almond, grape and the soy-bean. Tropical Asia has contributed the citrous fruits, rice, cottonseed, egg plant, black pepper, taro (dasheen, coco-yam), mango, mangosteen and endive. The islands of the Pacific and Indian Oceans were the source of coconut, bread-fruit, nutmeg and grapefruit. If there were no Ceylon we should have no cinnamon. Northern Europe and Asia comprised the birthplace of the edible varieties of apple, fennel, currant and gooseberry, while the mustard or cabbage family—turnip, rutabaga, cabbage, cauliflower, mustard, kohl-rabi, broccoli, Brussels sprouts—are indigenous to northern Europe.

From the region of the Caucasus Mountains we have obtained asparagus, quince, pear and plum. Asia Minor and the eastern end of the Mediterranean, where men made such early progress, is fairly well determined as the original home of wheat, barley, shallot, fig, date, English walnut, apricot, olive and artichoke. Garlic, although favored in Italy to-day, took its source from Tartary.

Southern Europe has contributed parsnip, celery, leek, chestnut, filbert, carrot and lettuce. The last may also have been indigenous to the Orient.

Africa has contributed no great quantity of foods, but their quality is good. Spinach is said to have originated in northern Africa, watermelon, cantaloupe and akee from tropical Africa and coffee from Abyssinia. The original home of the oat has been placed both in Abyssinia and the Danube River basin.

The new world has been no mean contributor. From North America come huckleberries, cranberries, pecans, hickory, pumpkin and possibly the kidney bean. Cocoa, corn, avocado, peanut, allspice, guava, vanilla, sapodilla, papaya, star-apple, cassava, chocho and sweet potato stem from tropical America, while pineapple, lima bean, Irish potato, tomato, maté and the herbaceous peppers found their origin in South America. A few foods were already so widely distributed in a cultivated or semi-cultivated form at the commencement of exploration that their original sources must remain unknown. This applies particularly to banana, plantain, ginger and yam.

Nor is the list complete. Within the last half century we have observed many new importations, particularly in our own country, where climatic conditions are so varied that both tropical foods and those that thrive in the cold northern climates may find suitable conditions for growth. The labors of the Bureau of Plant Importation, so delightfully described by David Fairchild in his memoirs, "The World Was My Garden," have made available within our own boundaries many of the most delectable of foods, especially those fruits indigenous to the tropics, such as mango, mangosteen, sapodilla, guava and akee. As time goes on these will undoubtedly come into more wide-spread use, as have their less perishable tropical cousins, orange, grapefruit, banana and pineapple.

The foods that we eat to-day stem from three general sources: (1) those indigenous to America; (2) those imported by the early colonizers from the older civilizations, which in turn had collected them from remote places; and (3) the newer tropical foods which are just making their start.

The history of the cultivation of foods parallels the history of the human race. However, it has not been until well within historical time that commerce and exploration have made such wide varieties so generally available. We read of the banquets of King Solomon and the extravagant feasts of Belshazzar, and of the Roman banquets, some of which are reported to have cost the equivalent of a thousand dollars per guest. Let us sit in at some of these meals.

The ancient Hebrews, who learned their cookery from the Egyptians, made quite a ceremony of their feasts. Three successive invitations were sent to each guest. When all were gathered together they sat cross-legged around a low table. The food was mainly a stew, since knives and forks were not available. The cut-up morsels were folded by the guest between slices of bread and eaten. The grease was rubbed from the fingers onto other pieces of bread, which were thrown to the dogs, waiting as anxiously as they do to-day. Servants were ready with pitchers of water for washing the hands. There were two persons to a dish. The food included flesh, fish, fowl, melted butter, bread, honey and fruit, four or five dishes in all.

The Greeks inaugurated the system of eating in a reclining position, while being sprinkled with perfumes to combat the odor of perspiration. They had two courses. The first was fish and meat, vegetables and entrées. The second, pastry and fruit, was followed by salty cakes, cheeses and the like to promote heavy drinking. This was accompanied by music, songs and slave dances, and garlands were entwined about the heads of the participants "to counteract the action of the wine."

The Romans learned cookery late. In 174 B.C. there were no cooks nor public bakers in Rome. The common people lived on a porridge made of pulse. There were several vegetables. Fish, domesticated animals and wild game helped out. The wealthy learned of the luxuries of the table from the Asiatic wars. They went mad on the subject of gastronomy. The best cooks were the most expensive slaves. The Emperor Vitellius, an enormous eater, sent his legions to every part of the empire to procure new and exotic foods. In a typical Roman feast the first course, merely an appetizer, consisted of conger eels, oysters, mussels, thrushes served on asparagus, fat fowls, shellfish and marrons. The second course had more fish, venison, wild boar and wild fowl. The third, or main, course included the udder of swine, boar's head, fricassees of fish, duck and other fowl, pastries and bread. Cheeses, lampreys, tongues of nightingales, brains of peacocks and flamingoes, mushrooms and the rarest vintage wines were served.

While Petronius' description of Trimalchio's feast is satirical, we may presume that the foods listed were the delicacies of the time. Also, he could not have mentioned any foods that were then unknown. We may therefore list some of the favorite foods of the days of Nero, as follows:

Meats. Sausage, beef, kidney, pork, bacon, lamb, lambstones, sweetbread, liver, chitterlings (present-day chitlings).

Seafood. Lobster, pilchard (sardine), mullet, sole, lamprey (an eel-like fish), snail.

Fowl. Wheatear, goose, capon, blackbird, pheasant, guinea, stork, thrush, peacock, gizzard.

Game. Hare, boar, bear.

Fruits. Damson, pomegranate, fig, date, apple, peach, grape, raisin, quince, olive.

Vegetables. Chickpease, pulse (a legume), scallion (shallot or onion), mustard, beet, lupine (a legume), turnip.

Seasoning. Pepper, vinegar, cumin (a spice of the caraway family).

Nuts. Almond, chestnut.

Sweets. Honey.

Dairy products. Hen's eggs, goose egg, cheese.

Confections. Tarts, custards, marchpane, junket, household-bread.

This was the day of the vomitoria, when the gluttonous banqueters stepped aside into special rooms provided for the purpose, emptied their stomachs and returned to start again. Perfumes, music, dancing, dice, gambling and votive offerings to the gods provided the divertissement.

There must have been considerable monotony to the diet. So many of our more delectable fruits and vegetables were lacking. There were no potatoes, tomatoes, chocolate, vanilla, corn, peanuts, pecans, rice or coffee. The list is not complete. They lacked many of the spices which are so popular to-day. According to story, garum was their favorite sauce. This was made from the entrails of fish allowed to ferment until liquefied, sort of a prehistoric Worcestershire sauce or anchovy paste. This story was told by Horace, who was the cartoonist of the day and inclined to exaggerate. It may not be quite true.

The Britons learned cookery from their Roman conquerors and from Germanic immigrants.

In the Dark Ages, all Europe forgot how to cook. Charlemagne's banquets were barbaric affairs, with never more than four dishes, chiefly spitted meat. With the Crusades the art was reintroduced, again from the East. The Medici of Florence were chiefly responsible for the renaissance of cooking. Catherine de Medici introduced it into France, where, from the point of view of the epicure, it has remained paramount ever since.

Such, then, is the story of why we eat what we are eating to-day. It is the thrilling history of man, responding first to necessity, later urged on by the need for availability and convenience, and subsequently developing the urge for new tastes and for greater palatability of his sustenance. It is the story of patient husbandry through the ages, of disease and death following trial and error, of avarice, thievery and war. It is the story of exploration and discovery. When, to-day, we complain that our soup is not properly seasoned, that our melons are not sweet enough, when we complain of the dryness of our grapefruit or of the sogginess of the sweet potato, let us, instead, give thanks to those unsung heroes of the past whose exploits have made it possible for us to sit each day at dinners such as were never dreamed of by the epicures and gluttons, kings and emperors of bygone days.

VIEWS ON MACHINERY AND UNEMPLOYMENT

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I

In 1821 the French economist Jean Baptiste Say wrote a series of "Open Letters" to his English contemporary, Thomas Robert Malthus. In one of these letters Say declared that "We who scribble paper in search of truth must be on our guard: if our writings should go down to our grandchildren, the terror with which we contemplate improvements which they will have greatly excelled, may probably appear to them somewhat laughable."[1] This statement suggests that the problem of "the machine" is not of recent origin. It antedates by many years the appearance of "Technics and Civilization," "The A.B.C. of Technocracy" and "Successful Living in This Machine Age." The statement indicates, moreover, that the early economists were interested in the mechanization of industry and its social and economic consequences—and felt impelled to say something on the matter.

Any discussion of "Views on Machinery and Unemployment"—or, more correctly, "Views of Economists on Machinery and Unemployment"—must, therefore, if it is to be at all comprehensive, take us back at least to the time of Say and Malthus. Indeed, it must take us back even farther, for the problem of "technological unemployment," to use the modern term, was dealt with by economists for half a century or more before Say wrote his letters.

If serious discussion of technological unemployment goes back that far, to the second half of the eighteenth century, the phenomenon itself dates back very much farther. There is ample evidence to prove that for centuries people have known about, have feared and have tried to forestall the labor-displacing effect of machines. The discouraging experiences of William Lee (d. 1610?), who figured out a way of knitting stockings by machine, and of William Ged (1690–1749), who invented a stereotyping machine, can be cited as partial proof of the contention.

But technological unemployment as a really serious social problem is not so old. Not until the first stages of the industrial revolution did it take on such a character. While men had been displaced by machines long before the revolution started, it was not until what one might call the "formal" beginning of that tremendous process of change, during the latter part of the eighteenth century, that the volume of such displacement became large, and the amount of comment and controversy centering around it became appreciable. In taking up our subject, we shall not be guilty of any serious omissions, therefore, if we restrict ourselves to the "New Era" that began slightly more than one hundred and fifty years ago.

Our method of approach will be primarily topical in nature, rather than chronological; that is, we shall examine various opinions—past and present—on the relationship between machinery and unemployment in terms of certain of their principal features and not according to the date of their appearance. Within each topical section, however, we shall generally proceed in chronological fashion.

II

In their discussions of technological unemployment the early economists emphasized the effect of new machinery on the price of, and the demand for, the article produced. "The introduction of

[1] Letter No. 4, "Letters to Malthus," 1821.

machines," said Sir James Steuart, the Scotch economist, back in 1770, "is found to reduce prices in a surprising manner: And if they have the effect of taking bread from hundreds, formerly employed in performing their simple operations, they have that also of giving bread to thousands, by extending branches of ingenuity, which without the machines, would have remained circumscribed within very narrow limits."[2] Fifty years later Mrs. Jane Marcet, probably the earliest popularizer of economics and author of one of the most amusing books in the whole field of economic literature, affirmed that "when any new machine or process whatever which abridges or facilitates labor is adopted, the commodity being produced at less expense falls in price, the low price enables a greater number of persons to become purchasers, the demand for it increases, and the supply augments in proportion; so that it frequently happens that more hands are eventually employed in its fabrication than there were previous to the adoption of the new process."[3] And still later Nassau William Senior, a very capable English economist and professor at Oxford, stated, in terms more emphatic than Mrs. Marcet's, that "The usual effect of an increase in the facility of producing a commodity is so to increase its consumption as to occasion the employment of more, not less, labor than before."[4]

Running through most of the early treatments of machinery and unemployment and continuing down to the present day, one will find the reduced-price, increased-demand argument. New machines lead to lower costs of production. Lower costs make possible lower prices. And at lower prices more units of the article will be sold.

[2] Quoted in Arthur Young, "Political Essays," pp. 214–5, 1772.

[3] "Conversations on Political Economy," p. 114, third edition, 1819.

[4] "Political Economy," p. 166, third edition, 1854.

The new machinery, according to the old economists, made possible lower prices. But lower prices were more than a *possible* outcome of the use of the new machines: they were generally the *actual* outcome. The early writers gave little attention to the possibility of prices not being reduced in keeping with the lower costs, and for a very good reason. At the time they wrote, competition was very keen. The manufacturing and the distributing of goods were for the most part carried on by many small business units, no one of which had any dictatorial control over the prices charged. In other words, the competition was atomistic in nature. Prices in the market were competitive prices. They were sensitive to changes in costs, as well as to changes in demand. The introduction of cost-reducing machines, therefore, was likely to be reflected very soon in lower prices for the goods produced. The lower prices would stimulate buying. With more units being demanded in the market, more would be produced. Men would be needed to make the extra units, and in this way the amount of displacement would be kept at a relatively low level.

The situation during the eighteenth and nineteenth centuries was much different from what it is to-day. At the present time, with monopolistic control of one sort of another so widely established, many prices are rigid, fixed more or less arbitrarily by administrative decree rather than by the operation of competitive forces. Under such a condition costs may fall as a result of the introduction of labor-saving machines, but prices may remain stationary. When this happens, the amount of technological unemployment is rendered greater, because, with prices the same as they were before, the volume of consumer-demand remains unchanged. The potential market that could be tapped with lower prices continues to exist in a state of dormancy.

That is what happened, for example, during the twenties. Costs were re-

duced in many industries, but prices did not drop commensurately. According to calculations made by the National Bureau of Economic Research, the wholesale price level of consumers' goods dropped from an index of 100.4 for the years 1921–1925 to 100.0 in 1929 (the base year).[5] With costs going down and prices remaining relatively fixed, it is not surprising that profits went up. Prof. Frederick C. Mills has estimated that the aggregate profits of 2,046 manufacturing corporations increased by 90 per cent. between 1922 and 1929. From a base index of 100 in 1922 they expanded to 190.3 in 1929.[6]

With so much of the pecuniary gains of technological improvements going to the profit-receivers, rather than to the consumers, new jobs for the workers displaced by the improvements came into existence rather slowly and in what one might call a circuitous manner. This helps to explain the presence of a relatively large amount of unemployment during the last few years of "the prosperous twenties."

Present-day students of technological unemployment are greatly concerned about inflexible prices. They realize that the disturbing effects on labor caused by improvements in methods and machines are greatly enhanced when prices fail to decline in keeping with lower costs. One of their number has gone so far as to suggest that at such a time, and that as far as origin is concerned, "price unemployment" would be a more accurate term than "technological unemployment."[7] This suggestion is of questionable merit—since, if it were adopted, it would seem to be just as logical to call seasonal unemployment that is due to a failure of employers to dovetail their products, "dovetail employment!"—but the fact remains that price rigidity is an extremely important factor in determining the amount of unemployment caused by technological changes.

In addition to inflexibility in the commodity price-structure there is at the present time no small amount of inflexibility in the price of labor. Wages are not as pliable as they once were. Since the days when Steuart and Malthus and Senior wrote their books the free play of supply and demand forces in the labor market has become increasingly restricted. Both trade unions and governments have interfered more and more in determining wage levels. Whatever benefits may follow from this sort of action, and it must be granted that they are not inconsiderable, it is an undeniable fact that controlled and inflexible wages may intensify the seriousness of technological unemployment. They may act as a direct incentive to employers to introduce additional machinery.

III

From the statements already quoted it is clear that the older economists were well aware of the fact that machines might decrease the demand for labor in particular occupations and that the displaced workers might experience inconvenience and suffering. Even the optimistic Mrs. Marcet admitted that "The invention of machinery . . . is often attended with much partial and temporary inconvenience and hardship."[8]

While they agreed that the demand for workers in specific trades or occupations might decline as a result of mechanization, the earlier economists—with a few notable exceptions—did not believe that the *total* demand for labor would ever drop.

David Ricardo, better known as one of the outstanding members of the so-called Classical School of Economists than as a wealthy stock broker and a member of parliament, was one of the exceptions.

[5] "Statistical Abstract of the United States," p. 312, 1938.

[6] "Economic Tendencies in the United States," p. 399. It should be added that in 1922 business was just recovering from the first post-war depression.

[7] See *The American Economic Review*, p. 251, June, 1939.

[8] Marcet, *op. cit.*, p. 114.

Reversing an opinion he once had held, Ricardo, in his famous chapter "On Machinery"—which he added, in 1821, to the third edition of his "Principles" —maintained that the substitution of machinery for labor was "often very injurious to the interests of the class of laborers."[9] The view held by the laboring class "that the employment of machinery is frequently detrimental to their interests, is not founded on prejudice and error, but is conformable to the correct principles of political economy."[10]

According to Ricardo's opinion, it was quite possible for machinery not only to decrease the demand for particular types of labor, but to decrease the total demand as well. It could do this by causing a decline in the "gross annual produce" of the nation. Such a decline, Ricardo affirmed,[11] could not be brought about "in any other way but by a diminished employment of the industrial classes." Stated in another way, Ricardo believed that machinery, by diminishing the gross product of industry, could lead to an inadequacy of real purchasing power, which, in turn, would cause a scarcity of jobs.

Ricardo had great confidence in the soundness of his view. That machinery might decrease the total demand for labor appeared to him—he told Malthus[12] —to be "absolutely demonstrable," and in the chapter just referred to he attempted to give such a demonstration. The demonstration was not very convincing, however, and most of Ricardo's fellow economists, as well as most of those who came after him, either failed to subscribe to its logic or went only part way with it.

John Ramsay M'Culloch, a very close intellectual follower of Ricardo and the writer of numerous works on economics, thought it was very plain that every improvement in machinery increased the total demand for labor—thereby disagreeing on at least one point with his master! M'Culloch said that one might imagine a case where machinery had the opposite effect, where it caused a decrease in the total demand for labor, but he felt it could be safely asserted that such a thing had never occurred, and furthermore, that it was extremely unlikely that it ever would occur. He admitted that new machines might decrease the demand for workers in certain occupations, but he did not believe that such a result was serious. "It sometimes, no doubt, though rarely, happens that the introduction of improved machinery is injurious to the laborers in particular departments, and that it sometimes obliges a greater or smaller number of them to change their employments." But M'Culloch—of whom, among economists, "it seems to have been given to carry everything to an extreme"[13]—thought that in the majority of industries this change of employment would not be such a great hardship as might at first be imagined; for businesses, he tells us, have "for the most part many things in common" and "an individual who has attained to any considerable proficiency in one, has seldom much difficulty in employing himself in another."[14] It is doubtful if any economist has ever made a more exaggerated statement concerning occupational mobility.

Malthus and Senior also took exception to Ricardo's view. Malthus did not believe that machinery rendered labor superfluous. When that happened it was a result of population increasing more rapidly than production. Senior said that he did not believe there existed "upon record a single instance in which

[9] "Principles of Political Economy and Taxation," (Gonner ed.), p. 379.

[10] *Ibid.*, p. 383.

[11] "Letters of Ricardo to M'Culloch" (ed. by Jacob Hollander), p. 114.

[12] "Letters of Ricardo to Malthus" (ed. by James Bonar), p. 184.

[13] John Davidson, "The Bargain Theory of Wages," p. 182.

[14] For the quotations from M'Culloch see his book "The Principles of Political Economy" (4th ed., 1849), pp. 209, 210, 214.

the whole annual produce had been diminished by the use of *inanimate* machinery."[15]

John Stuart Mill made some concession to Ricardo's opinion when he stated that he regarded as theoretically fallacious the view that the laboring class as a whole could not suffer even temporarily as a result of the introduction of machinery. But Mill distinguished between theoretical possibilities and real situations. "Nevertheless," he stated, "I do not believe that, as things are actually transacted, improvements in production are often, if ever, injurious, even temporarily, to the laboring class in the aggregate."[16] Mill recognized the fact that in individual cases machinery causes great suffering, and it was Mill who made the statement, to which all students of technological unemployment must sooner or later be exposed, that "There can not be a more legitimate object of the legislator's care than the interests of those who are thus sacrificed to the gains of their fellow citizens and posterity."[17]

John Elliot Cairnes, who has been called the last of the classical economists, to some extent echoed Mill's views on technological unemployment and to some extent he went still farther than Mill—farther towards "the left." Cairnes pointed out that changes from one method of production or from one system of industry to another are "almost always" (those are his words) accompanied by "more or less temporary inconvenience, and sometimes even with considerable suffering, for those whose occupations have been displaced." And then, after the fashion of Mill, he says "and this is a good reason for society doing all in its power to alleviate and repair these inevitable but transitory evils."[18]

Coming to American economists, one finds that their opinions bear a close resemblance to those of the English economists. Certainly they are not less optimistic in their general tenor. Prof. Arthur Latham Perry, of Williams College, declared, in a widely-used text-book of the seventies, that "the application of machinery to all departments of production, and the introduction of improved processes of every name, can hardly in the first instance be prejudicial to any, and are sure ultimately to be beneficial to all."[19] Perry, it might be pointed out, was greatly influenced by the French economist and publicist, Frederic Bastiat; and the latter's doctrine of "Economic Harmonies" blossoms forth profusely in the words just quoted. Everything in the field of industrial technology was working together for good. The first impact of technological change could hardly be unfavorable to any one, and the ultimate effects are bound to be favorable to all!

Simon Newcomb, who in addition to his other great accomplishments was an economist of some repute, did not assume the optimistic attitude of Perry—he was well aware of the adverse effects of mechanization on particular individuals or groups—but he asserted that every decrease in the demand for labor in one direction, resulting from a cheapening of production, was offset by an increase in demand in some other direction.[20]

David A. Wells stated, in his "Recent Economic Changes," which was published in 1889, "as established beyond the possibility of contradiction," that "all experience shows that, whatever disadvantage or detriment the introduction and use of new and improved instrumentalities and methods of production and distribution may temporarily entail on individuals or classes, the ultimate result is an almost immeasurable degree of increased good to mankind in general."[21]

[15] Senior, *op. cit.*, p. 163.

[16] "Principles of Political Economy" (Ashley ed.), p. 97.

[17] *Ibid.*, p. 99.

[18] "Political Economy," p. 257.

[19] "Elements of Political Economy" (fourteenth ed., 1877), p. 126.

[20] "Principles of Political Economy," p. 390, 1885.

[21] P. 366.

Speaking generally, the non-Ricardian interpretation of technological unemployment, the view that while machinery might decrease the demand for workers in particular occupations, it could not decrease the total demand for labor, ruled among American economists of the nineteenth century as well as among the English economists.

And a very similar interpretation is adhered to by most economists to-day. While they may occasionally veil their real thoughts in an esoterical and misleading use of terms—as when Professor Mentor Bouniatian says that "however rapid it may be, technical progress can not give rise to unemployment or become in any way harmful to the economic life of a country"[22]—they all admit that machines displace men; that is, they displace men in specific trades and occupations, in specific plants and in specific parts of the country. But they do not admit that machines displace labor in general. In other words, they do not believe that machines are creating a permanent and growing scarcity of employment opportunities.

This does not mean, however, that present-day economists feel there is nothing to worry about as far as technological unemployment is concerned. A few may take that attitude, but the great majority are of the opinion that here is a problem that requires not only careful analysis but constructive means of control—"a situation," as Godwin long ago pointed out, "that requires kindness and soothing."[23]

IV

As was indicated before, the earlier economists believed that the workers displaced by new machines would not be out of employment for any length of time. Their unemployment would be of a short-run nature. In the long run they would be reabsorbed. "It seems there are certain speculators, who apprehend danger from the introduction of those machines which shorten labor. But if they sometimes distress the worker it is never for a continuance."[24] This was the view of an early French writer on the subject, and it was shared by most members of the classical school.

The short-run aspects of technological unemployment were not stressed by the earlier writers. This was due in part to the fact that in analyzing this problem—and the same can be said of their treatment of other problems—they were principally interested in the operation of underlying and fundamental forces, and how these, if left undisturbed, would work themselves out. Their approach to economics was essentially static in nature, based upon assumptions which made it possible for them, by the method of cold logic, to formulate general principles or laws. (Of Ricardo it has long been said, grant his assumptions and you must grant his conclusions.) They viewed economic forces as working towards and achieving balance. Their economics was of the equilibrium variety. They did not give a great deal of attention to the dynamic, day-to-day, disturbing changes.

During recent years, especially since the latter part of the twenties, when the present wave of interest in "the machine" first engulfed us, the short-run aspects of technological unemployment have been more and more emphasized. Numerous studies have been made of the immediate, as contrasted with the more remote, consequences of mechanization. The "fugitive" sources of information concerning labor displacement, about which Carroll Wright spoke a half century ago,[25] have grown into a very formidable array. Especially notable in this connection are the extensive publications

[22] *The International Labor Review*, p. 343, March, 1933.

[23] "The Inquirer," p. 196, 1797.

[24] Quoted in Arthur Young, *op. cit.*, p. 212.

[25] "The Industrial Evolution of the United States," p. 325.

now being put out by the National Research Project of the Works Progress Administration. With a great deal of additional factual material before us we are now in a better position to discover the true nature of our problem. But it is still necessary for us to make some use of the method of approach adopted by the old economists. *A priori* reasoning and long-run considerations are still extremely important. Popular writers may doubt their value, and even ridicule their use, but scientific students of the problem can not neglect them.

V

Against the background of the older views we have just passed in review, and on the basis of evidence we now have at our disposal, it is fitting in conclusion, to present a set of broad, general opinions concerning technological unemployment which as applied to the present day seem reasonable and correct.

(1) Technological unemployment is permanent if by "permanent" we mean constantly in existence. Technological improvements are always being made, though most of them are actually introduced into industry during years of prosperity, when business and employment are expanding. Before all the workers displaced by any one improvement are re-absorbed into new jobs, other improvements are made, and other workers are displaced. And so the process continues.

There is, then, what one might call an ever-existing pool of technological unemployment; a pool which has many inlets (there are many separate instances of labor displacement), but also many outlets (there are many openings in industry which "drain off" the idle labor), a pool which has a fluctuating level, but a level which never sinks to the bottom.

(2) When it is said that workers who are displaced by machines will in the long run be reabsorbed, two things should be kept in mind. First, the statement refers to the workers displaced by any given improvement; and hence it is quite consistent with the assertion just made about the permanency of technological unemployment. In the second place, "the long-run" is not an absolute magnitude. It can not be defined as accurately as can a pound, a foot or a British thermal unit. The term is relative in nature. Its duration varies from time to time, from place to place and from individual to individual. If general business conditions are poor at the time that the technological improvements are made, the long-run will be longer than if business is booming. If a given locality is in a decadent condition industrially, and if one of its continuing factories puts in new machinery which causes a number of individuals to lose their jobs, the long-run—the time in which these workers will find new jobs—will be longer than if new industries were growing in the community. If a displaced worker is slow in moving to points where jobs are available, or in equipping himself—or in being equipped—for a new line of work, the long-run is longer than if he were more mobile, and more adjustable. The long-run may thus be ten months or twenty months or thirty months or even longer.

Here, again, the factor of price should be mentioned, and emphasized. If prices are inflexible the duration of the long-run is extended and the whole problem of technological unemployment, as we have already seen, is rendered more serious.

The term long-run has been called "that face-saving invention of the classical economists!"[26] If it is used and interpreted with discretion the term can not be described in this way, for when it is employed in this manner it becomes a genuinely helpful analytical aid. It should be clearly understood, however, that the term is a relative one. Some of

[26] *The Forum and Century*, p. 327, June, 1939.

the confusion and disagreement that exists at the present time on the question of technological unemployment is due to a misunderstanding of what the expression means.

(3) The amount of technological unemployment in the country varies from time to time, depending on the rate at which technological changes are made and the rate at which the re-absorption process goes on. The situation may be compared to a race between two fictitious beings, "A" and "B." "A" represents the total labor displacement caused by technological changes, and "B" represents the total labor reabsorption. In this race "A" is always in the lead. "B" never catches up. This means, again, that there is always some technological unemployment.

The distance between the two contestants varies from time to time, however. Sometimes "A" takes a spurt ahead—perhaps some especially remarkable improvements are made in machine technology and new machines are introduced into industry in large volume, causing a great deal of unemployment. Then "A" may slow down for a while. For economic or technological reasons the speed with which new machinery is put into use slackens up. Perhaps the output of the inventors themselves temporarily diminishes. The products of their genius and hard work, it is important to notice, do not flow into the market in a steady and regularly increasing stream.

The rate at which "B" moves in the race is also irregular, but not nearly so much so as the pace of "A." The forces bringing about the reabsorption of the displaced workers are in more or less constant operation though they are compelled to act under various difficulties—rigid prices, labor immobility (both occupational and geographical), depressed business conditions, unutilized profits. To some extent "B" resembles the tortoise, in the story about the race between the tortoise and the hare. But it differs from the tortoise in two very important respects: its movement is not as steady, and at no time does it pass its competitor. In the race "A" never stops up completely as the hare did—technological improvements are always being made—and "B" is always some distance behind.

(4) Finally, the aggregate volume of technological unemployment is influenced by the ratio that exists between "old-product" inventions and "new-product" ones, between inventions that lead to what has been described as horizontal expansion in industry, that is, the expansion of branches of production that already are in existence, and those that lead to vertical expansion, or the creation of new industries.[27]

There is no immutable law which decrees that old-product inventions must lead to a constantly large and expanding body of techonolgical unemployment. But taking the situation as it exists today, with many prices inflexible in nature, and with a rather large degree of labor immobility, there can be no doubt that a predominance of old-product inventions leads to serious and possibly growing technological unemployment.

It is desirable, therefore, to have numerous new-product inventions. It is desirable. But is it possible? That is a question to which many individuals are giving a rather emphatic negative answer. Perhaps their answer is the correct one, though one may doubt it. However, if it is correct, then it seems certain that the amount of technological unemployment in the future will be larger than it has been in the past, unless, of course, prices become more flexible and constructive steps are taken to hasten the reabsorption of the displaced workers.

[27] See Professor Emil Lederer's article entitled "Is the Frontier Closed?" in *Social Research*, May, 1939, and the discussion that follows it.

TELEPATHY—A SURVEY

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In the last few years there has been a revival of interest in the subject of telepathy. Recently conducted tests have become widely known and discussed. It may therefore be of interest to present a short survey of the whole subject, including a few of the clever deceptions practiced and reviewing the psychological and other work which has already been done, and to try to draw some rational and definite conclusions therefrom. It might be well to say that the writer of this article has amused himself for many years with sleight-of-hand and legerdemain and has even perpetrated some amateur séances on his friends; so that he can verify from personal experience most of the statements that are here made.

The present interest in the subject seems to have started with two magazine articles[1] written about the tests conducted at Duke University. These articles were written in a popular way and brought the tests to the attention of the general public. It is doubtful whether such a very wide-spread interest could have been stimulated, if the two articles mentioned had stopped at merely describing the tests. The articles, however, made the statement that the Duke tests have definitely and positively proved that such a thing as telepathy actually exists.

The subjects of telepathy and clairvoyance have always appealed to the popular imagination, and a highly sensational statement of this kind, coming from an authoritative source and backed by apparently reliable tests, probably accounted for the exceptional interest which was created. Nearly every one can relate wonderful happenings or coincidences that have occurred and which they can not account for, and many find telepathy a convenient and easy explanation. Perhaps we still have left in us some of the reverence for magic and mysticism that our prehistoric ancestors possessed. However, an opinion expressed by some casual individual is a very different matter from a carefully conducted test or investigation carried out with the purpose of proving or disproving the existence of telepathy.

It will probably be a surprise to most persons to learn that in the last thirty or forty years a great amount of investigation has taken place and that the Duke tests are neither new nor original; they are merely duplicating experimental work already done, particularly that of J. E. Coover at Stanford University. There have also been several elaborate questionnaires sent out and recorded in great detail. Naturally, to form an intelligent belief or disbelief in telepathy, we must not neglect the results already found and the deductions drawn from older investigations.

The definition of telepathy is not entirely clear. A better term would be "thought transference"; and even this is not entirely satisfactory, for it does not define the mode of thought transference. Crystal-gazing, dreams of certain types, death warnings, premonitions that come true, automatic writing, etc., may be considered as a mode of thought transference. Then, too, telepathy is constantly confused with clairvoyance. For example: In the Duke tests, where a num-

[1] Articles by E. H. Wright, professor of English literature, Columbia University, published by *Harper's* for November and December, 1936.

ber of cards were placed on a table, face down, and some one tried to name them correctly, telepathy was not concerned. This would be a case of clairvoyance. There was no thought transference from one person to another. Yet if another person were standing by who knew the positions and what the cards were, then telepathy might have come into action; for the first person might have told the cards by reading the mind of the second person.

It is hard to see how clairvoyance can be explained without considering that it is supernormal. No "natural" explanation will suffice; and so spiritualism and other supernatural explanations have become established beliefs and with some persons virtually religions. Telepathy, on the other hand, does not necessarily transgress natural laws. It is possible to conceive of a "natural" explanation.

The *Carnegie Technical Magazine* for January, 1939, carried an article entitled "A Scientific Basis of Mental Telepathy." This article took the ground that it is an established fact that the process of thought is accompanied by the generation of electrical impulses in the brain; and that the brain might be considered as broadcasting a type of radio wave, which might be called a thought wave. If this wave were received on another brain in tune with the first, the wave might reproduce the thought. There is nothing in such an explanation to conflict with the conservation of energy.

While there are some objections to such a theory, nevertheless the existence of telepathy does not appear to be a logical impossibility; and for this reason scientists seem to be more interested in the demonstration of its truth or falsity than in that of spiritism. This article therefore considers only evidence that is for or against telepathy, and leaves out of consideration the whole field of clairvoyance.

In surveying the field of telepathy one is impressed by certain outstanding facts. First, that all mind-reading demonstrations which have been performed on a public stage and which have been properly investigated have been found to be clever pieces of deception; and second, that all such demonstrations given privately which have been properly investigated have been found to be either fraudulent or, if the performer is sincere, have been explained by some abnormal condition or influence.

The public demand for that which is marvelous has been with us all through history and is responsible for such a man as Cagliostro; but it does not follow that if Joan of Arc had hallucinations she was insincere. In the past fifty years science has undoubtedly taken a good deal of the mysticism out of the world. As the public wants mysticism, their demand may account for the increase in the number and for the particular type of mind-readers that have developed.

Let us consider a few representative examples. Many public performances are given by conjurers and magicians, who do not claim anything more than a clever piece of deception and so are not convincing; but in many private performances the performer definitely claims that he actually is a mind-reader and is often able to demonstrate his claim in a most convincing way. A good illustration is the following: A few years ago there was a certain Chicago medium who would permit any one to write in his presence the names of six persons, known to the writer but unknown to the medium. Of the six, one was to be the name of a dead person, the rest of living persons. These names could be written in any order or way the writer chose. The remarkable part was that the medium was able to select the dead name from the others.

In almost any case of deception there are certain conditions imposed, a careful study of which will sometimes lead to a solution of the mystery. Here there were

no conditions. The test could be performed with any paper or pencil, at any time or any place, without regard to surroundings, kind of light or other restrictions. When this is well done and worked up with proper explanations and supposed reactions felt by the medium, it is a most effective and amazing demonstration of mind-reading.

A detailed description of just how this is accomplished is given by David Abbott in his book, "Behind the Scenes with the Mediums." Suffice it to say here that the secret lies in the length of the pause occurring as the different names are written down. Before starting the medium asked the writer to get in mind the name of a dead person, and then started him writing before he had time to think up the names of the living persons. At the proper time the medium would push and hurry the writer, who in his desire to write quickly would put down that which came easily to his mind; namely, the dead name. There was naturally a hesitation before writing the unthought-of names of the living persons.

Such mind-reading demonstrations not only deceive the public, but the trained man may easily be led to believe that there must be an element of truth in it somewhere. Sir Oliver Lodge was one of these notable examples. In the past there has been written a great deal of supposedly scientific literature about such demonstrations. It is of practically no value, because very few researchers have given the time and effort necessary to become familiar with the methods used in such deceptions.

There is one type of public performance where written questions are held in the pocket of some writer in the audience and later answered from the stage. Probably the best known of such mind-readers was Anna Eva Fay, who had a considerable vogue and following a few years ago. She is largely forgotten to-day, but every now and then somebody tries to revive this type of mind-reading. So it may not be out of place to give a few hints about the methods employed.

The ushers in the theater pass pads to the audience to write their questions on. These pads have the second sheet prepared with a very light, invisible rubbing of paraffin. After the top sheet containing the question is torn off and kept by the writer, the pads are collected and taken into a secret room, where the pencil impressions left on the paraffin sheets are developed by rubbing with plumbago.

The performer, blindfolded, answers the questions from the center of the stage, where he remains in full view of the audience throughout the readings. In the case of Anna Eva Fay the questions were secretly telephoned to her. She had metal plates fastened to the soles of her shoes. Coming up through the stage and flush with its floor were two wires on which she could stand to make the necessary connection. Underneath her dress other wires were carried from the metal shoe plates to a small telephone receiver concealed in her hair close to her ear.

As to the questions themselves, there is a well-known technique for answering them. The following illustration is taken from a book by Burling Hull on this subject. The questions of course are signed and the performer gives the writer's name and asks him to stand. Here is a typical example:

Performer: You are thinking of taking a trip, isn't that so?

Writer: Yes, it is.

P.: Do you know any one whose initials are F. H.?

W.: Yes, I do.

P.: It seems as if you were planning a trip to Chicago. Is not that so?

W.: Yes, that is correct.

P.: I want you to think hard of the person whose initials are F. H. Is his name Harris?

W.: Yes, it is.

P.: Is his first name Frank? Tell me if I am right.

W.: Yes, that is right.

P.: Well, when you get to Chicago you are going to see Frank Harris and you will find everything satisfactory.

It seems to the audience as if the performer had picked these facts out of the air. In reality it is merely the way that the information contained in the question itself is presented. The question was: "Will I meet Frank Harris when I get to Chicago?" All the information has been given by the question; and although the writer himself knows this he is impressed as well as the audience, because he can not understand how the performer learned what he wrote.

Any facts or information that can be gotten about persons in the audience is used effectively. Names are looked up in city directories, well-known persons are often in the audience, theater employees are placed among the audience to overhear conversations, etc.

Of late years some wonderfully clever demonstrations of mind-reading have been given. Some of the methods employed by Burling Hull are extremely ingenious and impossible to detect. However convincing they may seem, all such demonstrations can be run down, and when properly investigated are easily explained; but in some cases it may take an Abbott or a Houdini to do this. So what wonder that the average psychologist is completely baffled and does not know what to accept or reject as real? There are a few, but very few, who, like the late G. Stanley Hall, are not only psychologists but trained also in the art of the magician and conjurer.

Perhaps the most marvelous demonstration of mind-reading is where a person consulting a medium is told things about himself which the medium could have had no possible means of knowing, even to a secret of his past life that no one but himself knew. A demonstration of this type is known among the medium fraternity as a "reading"; and one medium will ordinarily rate another by how good a reading he is capable of.

In order to understand how such seeming impossibilities can be accomplished, we must remember that our attention, like our field of vision, is limited to one thing at a time. We can not distinctly see separated objects at the same time, for our field of vision is too narrow; we must concentrate on one object. Similarly, we can only concentrate a strong attention on one thing at a time. Instances are well known where a soldier going into battle has been shot, in the arm say, and it was some time before he was aware of it. His attention was so absorbed by what he was doing that all other feelings and interests were crowded out for the time. Several emotions or interests can not exist together; the strongest submerges all the others and will predominate.

Furthermore, when we strongly concentrate our attention on some particular thing or event about to occur, not only do we use ears and eyes, but all our faculties are concentrated on it. The consequence is that we are so absorbed in it that we know nothing of anything else that is taking place about us, and will have missed the details leading up to the event that we are so intensely interested in. We would be unable to describe anything but the watched-for event. Extraneous happenings would have passed unnoticed, and it would be impossible for us to give an accurate description of what took place.

The art of legerdemain is based on this principle. It is impossible for any one to describe a piece of sleight-of-hand or a stage illusion, unless they know how it is done, because they describe only that which they put their attention on; and the illusion is so designed that they must put their attention on certain things in order to understand what is taking place. They see only what they are supposed to see. Their attention is controlled.

To come back to the medium and the sitter, as the visitor is called. In the first place, the sitter's account of what occurred is entirely unreliable. If the

medium was very skilful, he had so engrossed the sitter's attention with what was about to develop in the conversation that the sitter was unaware of what he himself had said. Actually, the sitter supplies information to the medium. For example: The sitter and the medium are alone in a darkened room, sitting opposite each other with a small table between them. The medium speaks rapidly and incessantly, and with a rising inflection. One can not tell whether he is making a statement or asking a question. He may say, "I feel that you are thinking of a child?" Always with a rising inflection, "I take it to be your child?" It may happen that the sitter has lost a child, and under such circumstances may become emotionally excited. If so, some sort of exclamation or reply is sure to come from the sitter. Remember the room is nearly dark, and dim phosphorent moving objects and vague sounds may be used to enhance the weird effect.

It is natural to reply to questions, and sooner or later people do. The reply is immediately seized upon by the medium and followed up with a volley of questions. This has given him a lead, and he pumps along this opening. After a little it is quite possible some startling or emotional statement may come out, the content, meaning and development of which has so absorbed the sitter that the detail of the conversation has gone unnoticed and the impression afterwards left is that the medium told these things.

It takes a strong skeptic not to give an answer to some of this questioning; and if the sitter is easily affected emotionally and in addition thinks, "Perhaps there might be something in this after all," the results achieved are often truly marvelous.

Let us now turn to a consideration of what has been accomplished with experimental testing in psychological laboratories and elsewhere. Here we find scientifically trained investigators and can accept their work without question of their sincerity. if not from Duke!

Laboratory tests are generally carried out between two persons; one who submits himself to be tested and the experimenter who conducts the test, often a psychologist. The experimenter may conceal an object in his hand and ask the other to guess what it is. If the test is card-guessing, the cards may be laid face down on a table in full view of the person tested; or the experimenter and the cards may be a long distance away and his questioning done by telephone.

Such tests, however, are subject to certain classes of errors. To guard against them is the real problem that the experimenter is confronted with. Some persons are subject to hyperesthesia; that is, they are unusually sensitive and receive unconscious impressions, which the questioner may likewise be unconscious of giving. Such subconscious and involuntary indications come in many ways. Impressions can be given by sounds, involuntary audible whispers, tensions of the body, from light, etc.

The backs of playing or other cards may reflect the light differently enough to unconsciously impress a person who is very sensitive to such a condition. A questioner may unconsciously change the inflection of his voice, to always accompany a question about a particular object. Such inflections can be transmitted over the telephone, where persons are long distances apart.

An example of muscle indication is the old test nearly every one is familiar with, where an object is hidden and then a blindfolded person is brought into the room to find it. Two persons who know the hiding place rest their hands on each of his shoulders, and unconsciously and involuntarily push and guide him to it. There is also the famous case of Beulah Miller, who apparently possessed telepathic power. If some one would count slowly, she would stop him at a number

that had secretly been selected beforehand. She really reacted to the involuntary movements of another person who knew the number.

There is another class of errors or disturbing influences that affect the tests—the habits of mind of those tested, their likes and dislikes, the similarity of thought between persons of like culture and education, etc. For example: a person asked to choose one card in a pack of playing cards is influenced by many things, dependent on his type of mind, his reaction to the colors red and black, his preference for high or low numbers, etc.; so that his chance of naming any particular card is by no means 1 in 52. It has been found that the queen of spades is a favorite with many persons. If any one is curious enough to try, they will find that when persons are asked to give a number between 1 and 10, the answer 7 will predominate. Among geometrical figures the triangle seems to be the most common choice.

Again, experimenters have found that persons who are being tested will often be keenly sensitive as to whether or not they will be classed as normal. In all sincerity and unconsciously, they may modify their answers in the direction which they believe to be normal.

Speaking of experimental work generally, the principal thing which distinguishes the world of to-day from that of yesterday is the wide acceptance of the scientific method. It is the best means of exploring the unknown, and is infinitely more powerful than any method of abstract reasoning. We find to-day that our large industrial organizations have elaborate research laboratories and a trained staff of scientific workers. Suppose it were desired to improve certain properties of a steel alloy containing nickel, lead and tin. The scientific method says: Make a small sample in the research laboratory, put everything in the alloy as usual with one exception; *viz.*, replace the tin with copper. Test this new sample, and if it shows an improvement make another sample with still more copper but without any other change. In the above experiment, every factor has been controlled, and by changing one factor at a time we know positively what the effect is.

In theory we should be able to apply the scientific method to an inquiry into telepathy, but practically it is impossible because all the factors can not be controlled. Not only are detrimental influences and errors uncertain, but they may be unknown. One of the commonest mistakes is to draw conclusions from experiments where many factors are not under control. If one person of a test, say in guessing cards, is more successful than others, it proves nothing so far as telepathy is concerned. Why attribute the success to telepathy, when a number of other influences may be responsible for it? If we do not know what the reason is for success, it is just as logical and more so to assume that it is not telepathy. Why not some of the "natural" errors explained above, which have escaped observation?

So many precautions must be taken to exclude errors, and so much inconclusive testing has already been done, that the hope of ever being able to control so many uncertain and unknown factors is extremely slight. A scientific test is a controlled test.

Another method of treating experimental results is to compare them with mathematical chance. Suppose a bag contains one red and one black ball. If we draw one at random the chance of getting either is equal. But it is possible that we might draw ten times and get the red ball every time. Yet if we were to draw one thousand times we would draw about as many black ones as red. Mathematics states that as the number of draws approaches infinity, chance approaches certainty. Here we are no longer dealing

with individual tests, we are dealing with averages. If the tests are carried into thousands of trials, the averages will become practically constant, and if unaffected by errors will agree with what might be expected from mathematical chance.

An average, of course, can be affected by errors or outside influences, but not as easily as an individual test. In many thousands of trials, a few wrong ones do not appreciably affect the value of the average. Errors or detrimental influences would have to persist pretty much throughout to affect an average. In long tests this is not likely to happen. The same persons or conditions would hardly continue throughout. This method of comparing experimental averages with chance, whatever may be said against it, appears to be the best method we have and the one largely in use to-day. Let us look at some of the results found by it.

A huge questionnaire was undertaken about 1895 by the Society for Psychical Research. The object was to find persons who had experienced hallucinations; that is, who had ever had a vision or distinct mental image of a living human being, known to them, and that appeared before them without any apparent physical cause. In all 17,000 replies were collected, and of these 1,300 answered, yes. These 1,300 were then investigated further, to discover how many were death coincidences; that is, where the person seen in this vision died within twelve hours after the time of the vision. The result was 30 death coincidences.

So out of 1,300 presentiments or premonitions, 30 of them came true. This is 1 in 43. Now the death rate or probability that any given person will die on a given day was found from the insurance tables at the time to be 1 in 19,000. We would therefore assume that if 19,000 persons had premonitions of some one's death, only one coincidence would occur if chance alone acted. Telepathy was therefore considered proven to exist, as the results were (1900 ÷ 43) 442 times what should have been expected from chance.

Now, as a matter of fact these figures are very far from proving the existence of telepathy. The chance of death, as given by the insurance tables, was determined by considering all kinds and types of men and women. Therefore, if this is to be used as a standard of comparison, it can not be applied to any particular class of men and women. Manifestly the data collected was from a particular class, *viz.;* those who had experienced hallucinations. In other words, the data are not complete or representative; and these figures, if they prove anything, prove only that people of a certain type of mind will reply to such questions, and all the people who did not reply are not tabulated at all.

However, what is much more important, many of those who replied may have had, during their life, many other hallucinations. These may have been unimportant and for that reason forgotten. When mathematical chance is considered, failures as well as successes must be taken into account; and as the failures are not recorded in this investigation, it is absurdly incomplete.

Every one knows the tendency to be impressed by a successful prophecy and to forget the thousands of failures. We all know, and yet we forget the hundreds of prophecies that do not come true. How many times has the weather prophet failed; the business improvement prophet; the stock market prophet; all the way down to the palmist and fortune teller! The author of this article, not long ago, had a strong presentiment that something was wrong at his house; but on hurrying there he found every one well and everything all right. Probably the next day he would have forgotten all about it, but being interested in this subject he remembered it. So if a presenti-

ment comes to naught, people attach no importance to it and it is forgotten; but let some strikingly vivid or emotional coincidence happen, and it will be remembered always. To obtain a record of such failures is therefore an impossibility; and the Society for Psychical Research had really collected only the most meager part of the data needed. How could a committee who were conducting a supposedly scientific investigation base a belief in telepathy on such evidence as this?

Another well-known investigation is one that was instigated by Sir Oliver Lodge. Sir Oliver was an eminent scientist, but he was also a believer in spiritualism, to which any of his books on the subject will testify. Such, for example, is his "Survival of Man." He had lost a son in the World War, and as this had affected him profoundly, it may in part account for his belief. However much Sir Oliver may have known of other subjects, he knew nothing of the medium's art of deception. Read any of his descriptions of table lifting, for instance, and any one familiar with the medium's methods will be astonished at his lack of information. The consequence was that he was deceived for some twelve years by Madame Palladino and other mediums, until finally Madame Palladino was exposed at a special séance which took place at the house of Professor Lord in Cambridge.

Sir Oliver was therefore the type of man who would be favorable to a belief in telepathy. To try to prove its existence he made use of the British Broadcasting Commission to ask questions over the radio. A great many answers were received, some 15 to 20 thousand; but the investigation came to nothing and was finally abandoned by Sir Oliver, in spite of his interest and his desire for some definite results. And what else could have been expected? Only a certain type of person will reply to such questions, and answers received will only represent those who are interested and probably favorable to a belief in telepathy. Such results can not be compared with what might be expected from mathematical chance.

Probably the most reliable of all such studies was conducted by Dr. J. E. Coover, about 1917, at Stanford University. Every possible precaution was taken, and even some special investigations were carried out to see if certain influences were detrimental. The mathematical treatment was thorough and accurate, and any one will be repaid by a careful study of his publication, "Experiments in Psychical Research at Leland Stanford Junior University."

He tested many persons in guessing lotto blocks, playing cards and other objects; and in no instance, without exception, were the results more than what might be expected from chance. He shows many plots of the curve determined from experiment, superimposed on the theoretical chance curve, and it is surprising how close they are together. He says in his conclusions on playing-card tests: "The results of 10,000 guesses are negative. No trace of an objective thought transference is found."

The latest investigation has taken place at Duke University, and great publicity has been given it because it is supposed to have produced a proof of the existence of telepathy. The complete tests have not yet been published, but judging from what has appeared in the *Journal of Abnormal and Social Psychology* they are very similar to those of Coover, although much less extensive. The card-guessing tests in particular are much the same, except that the playing cards used by Coover were replaced by cards with special geometrical designs.

The mathematical chance or probability curve differs a little from that employed by Coover. Any one interested will find a discussion of it in an article

entitled "New Evidence for Extra-Sensory Perception?" published in the SCIENTIFIC MONTHLY for October, 1937.

The real criticism of the Duke tests is of the data used. They were not collected impartially, they are not complete, they are not representative, and of course can not be expected to agree with theoretical chance. The method used was to continue a person's testing so long as he made a good score; but when his good scoring fell off he was said to have lost his telepathic receptivity, and his testing was discontinued. That is, they collected the good trials and threw out the poor ones. Mathematical chance is not even given an opportunity to operate. Such procedure is merely "loading" the data and of course will give favorable results.

Any one who has done any experimental work knows that if a sufficient number of tests are made the average will become constant and any further testing will not change it. But in the Duke tests all scores that would modify the average are rejected. Under such circumstances it is absurd to use mathematical chance as a basis for comparison.

If the Duke tests show some individual repeatedly gives a high score, all well and good, it may be an astonishing fact; but it can not be compared with the results of chance. Averages must be compared to chance, not individual or selected cases. The reason averages are used is to reduce the effect of exceptional cases; and if only exceptional cases are considered, all reference to chance must be dropped.

Any exceptional case must be considered on its own merits, apart from chance. It must be positively demonstrated that no error or influence is acting. In a large number of the Duke tests, the cards were both seen and handled by the persons tested; and here is an opportunity for serious error. However this may be, to control every factor of a test is a practical impossibility, as already pointed out. This impossibility of proof in exceptional cases has led experimenters to adopt the method of averages. Any conclusion drawn where proof is lacking is merely an assumption.

The final conclusion regarding telepathy is clear. It can be very positively and definitely stated that there has never been any evidence produced which would warrant the belief that there is such a thing as telepathy. The most careful and reliable tests show no indication whatsoever of it. While of course this is a negative proof, yet after all it is a proof that no such thing as telepathy exists. We can positively say that no mind has ever yet communicated with another mind other than through ordinary sensory channels.

STUDIES IN MOSQUITO BEHAVIOR

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MOSQUITO larvae, or wigglers as they are commonly referred to, are often found in the students' aquatic collections. Frequently interest in their behavior prompts a series of questions which either begin with the adverb *why* or end with the preposition *for*. A recent summer school class in zoology composed largely of school teachers was no exception. The class reviewed a few elementary facts pertaining to the anatomy of the subjects, then gave their attention to a study of larval behavior.

It was observed that most of the larvae left their resting place at the surface of the water in response to vibrations, shadow, temperature, food or variable intensities of light. Without prompting, more than one student asked the question, Why? The student answers received as explanations for this behavior are largely responsible for this paper. Later in this paper I will describe certain of the apparatus (Fig. 1) and give the results of certain experiments.

I think that it is only necessary to quote from several responses written by students to fully justify and warrant a critical analysis of a common misconception of animal behavior. It is my intention to emphasize the futility of a teleological explanation of animal behavior.

Mr. R.: The mosquito larvae come to the surface of the water because of their inability to breathe under water. The lack of air causes a stimulation which sets up an impulse resulting in the larvae moving about in search of air.

Miss S.: Because mosquitoes go to the top of the water to get air which is necessary for their life, and the top of the water is always the lightest part, the mosquitoes naturally condition air with light. Therefore when the top of the tube was darkened and the bottom lighted the innocent mosquitoes thought the bottom was the top.

It is a well-known fact that the larvae of this species (*Culex pipiens*) leave the surface of the water when a shadow is suddenly cast over them. Differences of opinion, however, have been expressed concerning the factors influencing this behavior. Holmes[1] states that age makes little difference to shadow reaction, while Stanley[2] found that only 10 per cent. of the newly hatched larvae respond to a shadow. The percentage of reacting individuals reaches 100 at the age of seven days. My observations confirm, in general, the conclusions offered by Stanley.

It is, however, evident that age, temperature and frequency of casting the shadow are factors influencing the percentage of individuals responding. In testing shadow reactions the optimum temperature was found to be between 27 and 29 degrees Centigrade and the optimum age between seven and eight days. The number of individuals responding to a shadow repeatedly cast is directly proportional to the interval between casting of the shadow. The writer agrees with Stanley in the observation that the larvae respond negatively to moderately bright light at low temperatures, but react positively to light of the same intensity at optimum temperature. This reversal of tropism occurs at about 15 degrees Centigrade (59° F.). Not only were larvae observed to leave the surface if a shadow were suddenly cast over them, but they also left at the incidence of vibration. A knowledge of these facts together with the aforementioned experience suggested the problem

[1] S. J. Holmes, *Jour. Animal Behavior*, 1: 29–32, 1911; "Studies in Animal Behavior," pp. 50–120. 1916. Henry Holt Company.

[2] J. C. Stanley, "Light Reactions to Mosquito Larvae, *Culex pipiens*." Unpublished. 1931.

of attempting to determine the factor or combination of factors which are responsible for the behavior of the mosquito larvae with particular reference to surface orientation.

To test certain phases of the problem the following apparatus was designed (see Fig. 1). A glass tube 12 inches in length and 2 inches in diameter was sealed at one end and erected on a standard. This tube was fitted with a black paper sleeve 10 inches in length. The sleeve was in two sections permitting a separation at the center. The sleeve could be moved up or down on the tube. A Spencer microscope lamp was utilized as the light source and its rays could be directed to the top, center or bottom of the tube as desired. Within this tube light and temperature could be controlled and the varying effects studied on larvae of known age.

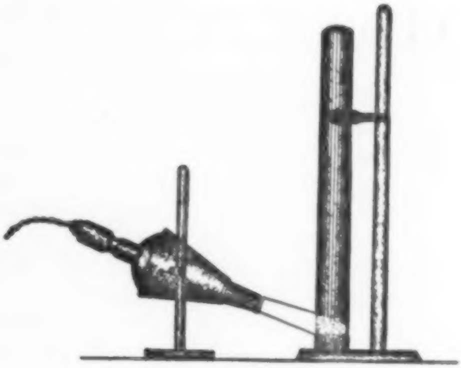

FIG. 1.

In the moderate light of the room or when the light of the lamp was directed at the top, larvae were to be found only at or near the surface. When the top of the tube was darkened and only the bottom illuminated, the larvae, one and all, migrated to the bottom of the tube. Here the young larvae remained and *died.* The older and proportionately larger larvae, after all observable movements cease, slowly float to the surface where they recover. It is assumed that this difference is due to a larger supply of air in the bodies of the older larvae, thereby explaining the difference in their specific gravity. During the experiment when the light was directed at the center of the tube, the top and bottom being dark, the larvae of all ages congregated in this area. This was a temporary orientation, in that movement into the light area was followed by a movement to the surface and back to the center. No larvae were observed to die in this latter situation.

In order to eliminate the air factor at the surface the following procedure was followed. Different age larvae were placed in test-tubes and attracted to the bottom with a beam of light. During their stay at the bottom of the tube liquid wax was poured into all but the control tubes. The wax upon hardening sealed the tubes, leaving no air space above the water. Larvae in the sealed tubes reacted similarly to those in the open tubes in their responses to light, vibration and temperature. The larvae in the sealed tubes died in a relatively short period of time.

Can it be possible that air, with its life-giving oxygen, is of secondary importance in determining surface orientation? If, on the other hand, light and gravity are major factors, what will be the effect of the absence of light from the time of hatching, upon surface orientation and feeding? Frost, Herms and Hoskins[3] state that the food factor is of major importance in rearing mosquitoes in the laboratory. Mr. William F. Bradley, a student of mine, volunteered his assistance in the feeding and rearing of the larvae. He was successful in obtaining close to 100 per cent. hatch and reached a maximum emergence of 86 per cent. Following is a summary of the feeding experiment, the food being Fleischmann's yeast in suspension in 2,000 cc of distilled water 12 inches deep. The tem-

[3] F. M. Frost, W. B. Herms and W. M. Hoskins, *Jour. Exp. Zool.*, 73: 3, 461–479, 1936.

perature was 28° C. (82° F.) and the illumination moderate.

TABLE I

FEEDING EXPERIMENTS

No. of larvae	Food per cc	Emergence of adults
50	0.15 mg	00 per cent.
50	1.00 "	86 " "
50	5.00 "	10 " "

The following is the summary of a feeding experiment conducted under a variety of light and food conditions, also in distilled water 12 inches deep and with 1 mg of Fleischmann's yeast to 1 cc of water at 27 to 29 degrees Centigrade.

TABLE II

Container	Emergence of adults	Light	Presentation of food	Maximum depth found
No. I	None	None	At bottom	5 cm
No. II	2 per cent.	None	In suspension	15 "
No. III	86 " "	Moderate	In suspension	23 "
No. IV	12 " "	Moderate	At bottom	30 "

From these observations it is evident that under laboratory conditions it is possible to obtain reasonable emergence when conditions are kept as close to the optimum as possible. It should be stated that there is no apparent difference in the percentage of hatching, whether the eggs are kept in the dark or under normal daylight conditions. It is quite evident, on the other hand, that the larvae kept in the dark had little chance of survival. Few larvae were found at any one time in containers kept entirely in the dark.

The above table indicates that light (and the absence of light) materially influences the survival through its effect upon behavior. Just how deep mosquito larvae will descend was not determined, but it should be noted that they are able to reach bottom (30 cm). When the food is in suspension, larvae are found at a depth of 15 cm when the container is dark, and 23 cm when the container is light. When the food is at the bottom, larvae descend to it (30 cm) provided the container is in moderate light, but in the dark a descent of 5 cm was the maximum observed. A comparison of the behavior of the larvae in container No. I with the activity of those in container No. IV suggests that, though food is within the physical reach of the organism it becomes available only to those individuals so activated by an antecedent stimulus, in this instance light.

Discussion: I do not wish to presume any originality for the description of the aforementioned responses of the mosquito larvae. I desire, however, to use this information to illustrate a fundamental principle of animal behavior, namely, that animal behavior is the ultimate result of stimuli upon a particular arrangement of anatomical structure. In the lower organisms many neural motor connections are made in such a way that the ultimate behavior is classed as a tropism. According to Loeb, a tropism is the orientation of the body with reference to the source of the stimulus. It is not difficult to find examples of this type of behavior having survival value. But such activities are remarkably stable and inflexible, and given the proper stimulation, the animal continues with a machine-like precision to the end, even though the ultimate result is fatal. The positive reaction of the larvae to light at the bottom of the tube where they suffocate, or in the absence of light their orientation to the surface where they starve are just two examples of the above-mentioned stereotyped behavior. The same may be said of the conventional moth and the flame or the reaction of the crayfish upon contacting warm water. (A crayfish will crawl into the warm water (100° F.), go directly to the bottom and suffocate; the animal is physically capable of withdraw-

ing from the warm water but without exception remains and dies.) The examples of behavior just described can only be interpreted on the basis of an inherited predetermined neural motor pattern and the organism's inflexible response to a specific stimulus.

Learning, or as it may be called conditioning, was found to play no part in the reaction of the larvae to shadow or to light. Larvae hatched and reared in the dark show the same percentage of response at different age levels as do those hatched and reared in the light. The fact that there is a gradual increase in the percentage of larvae responding is indicative of a growth and maturation of the behavior mechanism.

The writer disagrees with Stanley when he states that, "the movement of larvae to the top of the water is not a matter of negative geotropism but is clearly positive phototaxis." How can this explain the fact that the larvae rarely leave the surface *except* under lighted conditions? Larvae only travel to the bottom (30 cm) or even half way unless the container is in the light. True, as I have stated elsewhere, mosquito larvae are positively phototropic, but they are just as surely negatively geotropic. The presence of food and, more specifically, its distribution further influences the orientation of the larvae. Oxygen, food and water of a tolerant temperature are vital necessities, but more important than these in determining surface orientation are light and gravity. Movement beneath the surface may be initiated by an absence of light on the surface with an accompanying light below, by shadow, by food, by vibrations or by lowering the temperature.

In conclusion, it would be well to remember that in general the ultimate pattern of the behavior of an organism is the joint product of the stimulus and the specific physical constitution of the reacting individual. Further, that in many of the so-called lower organisms the behavior pattern is quite rigid, and even in the higher groups limited in its flexibility. It is a precarious statement that assigns purpose, or design, to organic phenomena without consideration of the physical involvements.[4]

[4] S. O. Mast, "Light and Behavior of Organisms," pp. 246–260. London: Wiley and Sons. 1910.

BOOKS ON SCIENCE FOR LAYMEN

CAN THE DISABLED EXERCISE?[1]

THOUGH there are innumerable books attempting instruction in athletics and sports for the normal and physically fit youth, there are few which comprehensively consider the problems of the physically handicapped. The need for such information is obvious, and the title of Stafford's recent book encourages one to feel that this need has at last been met. The majority of previous discussions of exercise for those with physical defects have been theses loyally defending some system of calisthenics with little consideration of the depressing emotional effects of such corrective exercises, or scientific analyses of the exercise problems arising in some specific malady or deformity. Mr. Stafford avoids the first of these limitations and attempts to be comprehensive. He frankly recognizes the need for pleasure and satisfaction in recreational activities. As a matter of fact, this need is stressed far beyond its true value; the athletic director fails to visualize much if any pleasure in life except through games or sports.

The outlined objectives are excellent. It is pointed out that such recreative activities should, if possible, attain (1) some corrective value for the particular defect, (2) a minimum of "expectancy of injury" and (3) definite recreational value. The recommendations as to how these desiderata may be attained are less inspired. Those sections of the book dealing with structural or mechanical handicaps, such as amputations, stiffened joints, postural defects and atrophies and paralyses consequent to infantile paralysis, are thorough and instructive. There are many suggestions valuable to physical instructors, parents of crippled children and school nurses. The failure to include the creative handicrafts as forms of exercise invaluable in muscle training and in rebuilding morale is a serious omission. Occupational therapy has long since proven its value in rehabilitation of the physically and/or mentally handicapped. Similarly serious is the total absence of any consideration of sports, games or exercises particularly appropriate for those with defects of vision or hearing.

In addition to these sections dealing with sports for the mechanically handicapped, the author ventures into consideration of the value of exercise and games for a variety of medical conditions. The problems of sports for those with heart disorders are most inadequately considered. While it must be admitted that there is nothing definitely harmful in the suggestions, there is much which may confuse and mislead the lay reader. Anxious parents are invariably prone to believe that which they want to believe. The elementary school discussions of endocrine disturbances were better deleted. A little knowledge can be a dangerous thing and that acquired by scattered reading particularly precarious. Those sections on exercise for the convalescent, for those with glandular disturbances and for those with heart disease are reminiscent of term papers by freshman biology students, devoid of the remotest conception of the significances, limitations and implications of the obvious citations.

EDWARD J. STIEGLITZ

[1] *Sports for the Handicapped.* By George T. Stafford. Illustrated. $2.75. 302 pp. Prentice-Hall, Inc.

A CATALOGUE OF POISONOUS PLANTS[1]

WHEN an author attempts to make scientific knowledge useful and under-

[1] *Poisonous Plants of the United States.* By Walter Conrad Muenscher. Illustrated. $3.50. xvii + 266 pp. Macmillan Company.

standable to the layman he is not always successful in creating interesting reading. In his recent book on poisonous plants Muenscher has combined information from a great many authoritative sources and has presented it in such a way that it *is* understandable and interesting. The work should prove to be of special interest to the cattleman, the agronomist and the agrostologist. However, the book is written for reference rather than for general reading and the plan followed makes it more usable to the botanist, perhaps, than to the layman.

In the foreword the author indicates clearly his view on the significance of toxic substances in plants when he states (without pausing to comment on his philosophy) that the most plausible hypothesis concerning the role of plant poisons is that they are products of, or "stages in," metabolic processes. Thus he divorces the commonly held teleological notion that *toxins are developed for protection* against biological enemies. If toxins are stages in metabolism, the physiologist might well ask if some of these substances are not the final stages or end-products of metabolism and are, therefore, wastes or perhaps, in some cases, food reserve.

No doubt the reader will be interested and surprised to learn, upon reaching the introduction to the second part of the book, that there are as many as 400 species of poisonous plants in the United States. Also he will be surprised to find that this large number of plants is treated in some detail on less than 250 pages of print. Such a concentration is achieved, in part, by very concise plant descriptions, augmented by well-executed, faithful, line drawings illustrating seventy-five of the species.

A unique feature of the book is found in Part I, where several classifications of poisonous plants are given, based on different criteria. After pointing out that certain families (of which there are nine) such as the Ranunculaceae, Solanaceae, etc., include a majority of the poisonous species and therefore might be classed as poison families, the author lists a number of representative plants according to the nature of their toxic principle. That is, whether the nature of the toxic principle involved is due to an alkaloid, a glucoside, resinoid, etc. Another classification is based on the physiological actions of the poisons: blood poisons, neurotic poisons, irritants, etc. Among other categories an imposing list (of both scientific and common names) is given of those species (98 in all) which are known to have caused skin poisoning, or dermatitis, together with the part of the plant which carries the toxic principle. Unfortunately, the reader who is interested in pollen sensitization will not find a list of the plants which are commonly responsible for hay-fever. The classification of poisonous plants is brought up to date by a listing of those species which recently have been found toxic because of their selenium-absorbing habit.

In the second part (which makes up the bulk of the book) there is a systematic treatment of poisonous plants by families. Of these, two are fern and the remaining ones seed-plant families. In nearly every case the guilty plant is described (sometimes too briefly), its habitat and distribution are given, the poisonous principle is discussed, symptoms of poisoning, and in some instances methods of treatment are outlined.

A few shortcomings in so far as the layman is concerned are discovered if one examines the plan of the book and the descriptive material critically with a view to evaluating its usefulness. For instance, not knowing the name of a plant suspected of being poisonous, the layman might experience considerable difficulty in learning its identity in this volume. Or, it would not be impossible for a layman to confuse a non-poisonous

species with a description of a poison one. Again, if the identification of a poison plant were desired and only the symptoms of a case of poisoning were known (as in cattle, for instance) it would be impossible to make safe and certain determinations. Obviously the book was never intended to be used as an aid in diagnosing cases of poisoning or as a handbook of symptomatology. However, one of the values of the book lies in the fact that symptoms are included in the discussion. The taxonomist will agree with the author of the book that a key to poison plants would be of little use unless a key to non-poisonous species were incorporated. It seems that the value of the book would have been manifold, however, if a key (in the absence of colored illustrations or photographs of each species) had been included.

While the treatment of each species and its poisoning effects in the main is consistently systematic, there are infrequent omissions of flower color and certain other descriptive features which are of great help in making identifications. When, as in the case of *Suckleya suckleyana*, *Ligustrum vulgare* or *Buxus sempervirens* (to cite some random examples), there is no description of flower color or flower form, nor an illustration either, the value to the reader is much lessened. Or, as in the case of *Hydrangea arborescens*, where there is no description of the plant nor the behavior and characteristics of the poison described, the reader is disappointed. To give space to those plants which cause indefinite and minor physical irritations, or those which by chance flavor cow's milk, may seem unwarranted to those readers who would prefer more information on the strictly toxic species.

There has been deliberate limitation of the material in the volume so that only those species are treated which fall into the category of what the layman usually thinks of as plants. The poison fungi (wisely), the bacteria (obviously of necessity), and the poisonous algae (because of their relative obscurity, no doubt) are not given a place in the volume. There are, of course, practical difficulties which preclude all these groups, yet a student might expect to find reference to some of them in a book bearing the title of this one.

In all, the book is very commendable for every reference shelf. There is an excellent bibliography, especially valuable since it brings together references to the widely scattered literature on the various aspects of the subject which has appeared since the publication of Pammel's noteworthy "Manual of Poisonous Plants" thirty years ago. The index is complete and highly useful, including, as it does, both common and scientific names and some subjects.

G. W. Prescott

TRIBUTE TO A SCIENTIFIC LEADER[1]

As a companion volume to the "Papers and Addresses" of John C. Merriam, the staff members and research associates of the Carnegie Institution of Washington have prepared this testimonial volume of papers dealing with investigations of current scientific problems conducted wholly or partly within the institution and illustrative of the principles followed by Dr. Merriam during the eighteen years of his presidency. These papers epitomize the past and present nature of the activities of the institution in the various branches of science. They, therefore, have an outstanding historical value, since they have been prepared by investigators who are authoritative specialists each in his own field. Their reports reflect not only their own accomplishments, but are an index of present-

[1] *Cooperation in Research.* By Staff Members and Research Associates of the Carnegie Institution of Washington. Illustrated. x + 782 pp. $4.50 and $5.00 (cloth). The Carnegie Institution of Washington.

day knowledge in many of the leading scientific disciplines. They also are a record of the results and effectiveness of varying degrees of cooperation among investigators. The fields covered are seismology, geophysics, astronomy, terrestrial magnetism, nutrition, genetics, embryology, plant biology, history of science, American history, Mayan civilization and anthropology, early man in America, paleobotany, whales, geology and paleontology. A chapter on cooperative research by the editor of publications, F. F. Bunker, affords a general survey of the policies and procedures followed in the institution. A chapter on transmuting science into conservation by N. B. Doury is a revelation of what leadership has done in saving the redwood forests. A closing chapter on John Campbell Merriam as scientist and philosopher by Chester Stock is an appreciation of Merriam's paleontological work and a presentation of his philosophy of life. This volume is a fitting tribute to a courageous, constructive and efficient leader and organizer of American science.

CHARLES A. KOFOID

DRAMA AMONG FOSSILS[1]

THE trustees of the Carnegie Institution of Washington have honored their retiring president by this reprinting of his publications, beginning with his doctor's thesis in 1894, written under Zittel in Munich, and continuing through his comments on the relation between research and organization of knowledge from his closing annual report. The four large volumes contain 221 papers arranged according to subject, all reproduced with their plates and text figures. They are classified by subject and include papers on the paleontology of Reptilia (24), Mammalia (16), the fauna of the famous tar pits of Rancho La Brea (13), the various fossil faunas of the Pacific states and Great Basin (33), paleontology and human history (18), invertebrate paleontology (6), general paleontology (2), geology (11), the life of past ages (13), history (5), biography (4), general addresses (17), problems relating to nature (30), research and publication (17), research and government (5) and Carnegie Institution addresses and reports (7). A bibliography lists his publications in chronological order.

The opportunity to view life in the upper levels of its evolution as revealed in the fossil record has come to few biologists in America of the passing generation with the fullness with which it came to Merriam. None has made that record more vivid and scientifically significant nor has any one given more sincere effort to interpret this drama to men who think in all walks of life.

The section on problems relating to nature exhibit this and also reveal another phase of activity in relating his scientific knowledge to the conservation of natural reseources and the utilization of the esthetic and educational values of our national parks.

The fields included in this assemblage of scientific work and the period in which the work was accomplished are of prime significance in the history of scientific progress in the United States.

CHARLES A. KOFOID

[1] John Campbell Merriam. Published papers and addresses. Illustrated. 4 vols. $4.50 and $5.00. viii + 2672 pp. Carnegie Institution of Washington.

THE EXECUTIVE COMMITTEE OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

STANDING: SAM WOODLEY, DR. ESMOND R. LONG, DR. BURTON E. LIVINGSTON, DR. J. MCKEEN CATTELL AND DR. F. R. MOULTON. SITTING: DR. OTIS W. CALDWELL, DR. HENRY B. WARD, DR. EDWIN G. CONKLIN AND DR. GEORGE D. BIRKHOFF.

THE PROGRESS OF SCIENCE

THE COLUMBUS MEETING IN RETROSPECT

EVERYTHING worth while appears to be accomplished only at the cost of great effort. Consequently it is not surprising that the officers of the association at the time of this writing (January 3) are still carrying a load of weariness acquired at the meeting in Columbus, Ohio, from December 27 to December 30. In those four days, 259 scientific sessions were held for the purpose of presenting the 2,154 addresses and papers which were listed on the programs. Obviously no one person could attend all the scientific sessions, for at times from 30 to 50 of them were being held simultaneously. Nor could any one be present at any considerable fraction of the 42 luncheons and dinners which were arranged for different scientific groups.

Obviously, there are two consequences of such an occasion as the great meeting of the association in Columbus. One is the general impressions it makes on those in attendance; the other is the sound additions that are made to established facts and their interpretations. Any impressions received at Columbus were from science and scientists as a whole, for through its 15 sections the work of the association covers practically all of science. In addition, 41 affiliated and associated scientific societies joined in the meeting; altogether, about 6,000 scientists were in attendance.

To me the dominant note of the meeting was that of serene confidence in nature and man. Although the world is under lowering clouds of war, scientists walk in the light. Although politicians are despairing of the future, scientists have no serious misgivings. They know that human beings constitute a young and highly adaptable species. They have no fears that wars will sap the biological vitality of the race, for science is saving a much greater number of lives than wars will destroy. In fact, the populations of western countries that have the most science, as well as the most destructive wars, have increased with great rapidity during the past century.

This optimism was not only a general undertone of the meeting, but it was explicitly and brilliantly expressed by Dr. Kirtley F. Mather, professor of geology in Harvard University, in his Sigma Xi address on "The Future of Man as an Inhabitant of the Earth." Dr. Mather was considering not merely decades and centuries but millions of years. From a mountain-top of science he looked back over the geological ages, and forward to a comparable future during which the earth will almost certainly be suitable as an abode for higher forms of life. Poets are not the only ones whose "souls catch sight of that immortal sea which brought us hither" or to whom "the meanest flower that blows can give thoughts too deep for tears."

Some of the esthetic aspects of science were delightfully set forth by Dean Marjorie Hope Nicolson, of Smith College, in her Phi Beta Kappa address on "Science and Literature." The poems of the Old Testament keep step to the majestic parades of the sun, the moon and the stars, and sing of the quiet delights beside the still waters. The epics of Homer and Vergil and Dante and Milton are heroic portrayals of nature and man, the fundamental subject-matter of science. As Dr. Nicolson explained, science has given literature some of its noblest conceptions, and literature has added its own beauties to science.

But not all the general addresses pertained to such vistas of time or to such esthetic aspects of science. This is a practical world, and a cruel world to

those who are not in harmony with its fundamental properties. The programs of several sections of the association and of many of its affiliated societies were concerned with the relation of man to his physical and biological environment. But the discussions were not limited to such questions; a considerable number of addresses were upon various aspects of the relations among men. Indeed, Dr. Wesley C. Mitchell, president of the association, chose as the subject of his presidential address "The Public Relations of Science," and Dr. Julian Huxley spoke on "Science, War and Reconstruction." Moreover, the Section on Social and Economic Sciences devoted a whole session to the "Effects of Science upon Human Beings." The effects considered in the papers presented at this program were not simply those on human beings as individuals but as members of society. Dr. Alan Gregg, director of the Division of Medical Sciences of the Rockefeller Foundation, delivered an address on the biological effects of science upon human beings; Dr. Isaiah Bowman, president of the Johns Hopkins University, delivered another on the social effects of science; and Mr. Lawrence K. Frank, of the Josiah Macy, Jr., Foundation, discussed the cultural effects of science. These speakers were more acutely aware of the ills that now beset mankind than were those who considered humanity with a longer perspective in time or only incidentally in connection with explorations of the physical and biological worlds.

In retrospect the meeting of the association at Columbus was an inspiring success. Speaking of the human side alone, it would be difficult to find in all our country an equal number of men and women who are more nearly normal, both physically and mentally, than were the 6,000 scientists who presented and listened to more than 2,100 addresses and papers in four days. With pleasant memories of such a wholesome atmosphere and outlook, no feeling of weariness remains.

F. R. Moulton

NEWLY ELECTED PRESIDENT OF THE AMERICAN ASSOCIATION

Active students of biology, especially those whose main interests lie in the field of plant science, have welcomed the recent election of one of their number to the presidency of the American Association for the Advancement of Science for the year 1940. Dr. Albert F. Blakeslee, the newly elected president, is an eminent botanist of broad interests, best known for his contributions to the physiology of development and reproduction in plants and to the special science of plant genetics. It is now thirteen years since a botanist was president of the association.

Son of Francis Durbin Blakeslee and Augusta Miranda Hubbard Blakeslee, President Blakeslee was born in Geneseo, N. Y., in 1874. He attended East Greenwich Academy, East Greenwich, R. I., where his father was principal, and then entered Wesleyan University, at Middletown, Conn., where he received the bachelor's degree in 1896. For the next four years he taught the sciences in Montpelier Seminary, Montpelier, Vt., and in East Greenwich Academy. He entered Harvard University in 1900, where he was instructor in botany at Radcliffe College and teaching fellow in botany at the university, receiving the Ph.D. degree in 1904.

In his student period at Harvard he received inspirational guidance from Farlow and Thaxter, who led him to undertake his now well-known study of the Mucors. Aided by a research grant from the Carnegie Institution of Washington, he spent the years 1904–06 in further study of the Mucors with Klebs,

DR. ALBERT F. BLAKESLEE

at Halle. It turned out that many of these simple fungi are heterothallic, each species existing as two physiologically different strains or races, called *plus* and *minus*, rather than male and female, and that zygospore formation results only from conjugation of the two strains. This discovery led to a great advance in our knowledge concerning the nature of the Mucors and also concerning one of the most primitive manifestations of sex.

After his return from Germany, Blakeslee held an instructorship in botany at Harvard and Radcliffe for one year and then became professor of botany in what was soon to develop into the Connecticut Agricultural College, at Storrs. He held that position—with subsequent change of title from botany to botany and genetics—for eight years, till the fall of 1915. In that period he showed that he was an excellent teacher and cooperator as well as a successful investigator. With his classes of forestry students he worked out keys for tree identification in winter, which led to a joint bulletin, with Dr. C. D. Jarvis, from the Connecticut Agricultural Experiment Station, on "New England Trees in Winter." The contents of that bulletin afterwards appeared in book form, as "Trees in Winter." In some attempts to hybridize tree species Blakeslee was successful at a time when little attention had been devoted to that field of study; among other hybrids produced was one from Austrian pine and Japanese pine, one of the very first hybrid pines to be secured in this country.

In a waste place at Storrs Blakeslee developed an agricultural botanic garden on original lines, which he described at a symposium held by Section G of the American Association at its Boston meeting in 1909, in a paper entitled "The Botanic Garden as a Field Museum of Agriculture."

On leave from the Connecticut Agricultural College, he spent the year 1912–13 at the Department of Genetics of the Carnegie Institution of Washington, at Cold Spring Harbor, Long Island, in further studies on sex reactions in the Mucors. In the fall of 1915, he became investigator in plant genetics in that department, where he has remained ever since. When he accepted that appointment he says he intended it to be for only a few years, because he "liked teaching and planned to get back to it." That he never did, but every summer he has conducted a seminar for a large number of assistants. Despite his long devotion to intricate research problems, he still loves nothing better than to explain and interpret experimental results and to discuss his thoughts with all who are interested.

The long series of remarkably productive experimentation on jimson weed for which Blakeslee and his colleagues, cooperators and assistants are so well known, not only in genetics but also more recently in physiology and horticulture, appears to have derived from those early efforts to set up at Storrs a living agricultural museum. A batch of seeds of *Datura stramonium*, received from the U. S. Department of Agriculture among samples of economic weeds, produced both purple-flowered and white-flowered strains, which served well to demonstrate Mendelian inheritance. The "Globe" mutant of this species, found at Storrs in the summer of 1915, was studied after Blakeslee's removal to Cold Spring Harbor, and additional mutants soon appeared. Although many plant species were cultured and examined in the next few years, as possibly suitable for intensive study in genetics, Datura seemed most promising in various respects, and it soon became the sole object for thorough investigation in Blakeslee's new experiment garden and laboratory. Staff members, assistants and a number of able cooperators all joined in those Datura studies, with many new and important results. Unusually good facili-

ties for experimentation were at hand, and Blakeslee's natural penchant for trying new experimental treatments of various sorts seems to have been thoroughly gratified.

Much attention has recently been given to the doubling, quadrupling, etc., of chromosome numbers and to the accompanying physiological and morphological changes in Datura and other plants induced through application of the alkaloid colchicine. This is derived from bulbs or seeds of species of Colchicum (sometimes called autumn crocus by gardeners) native in the region of ancient Colchis—of golden-fleece fame—east of the Black Sea. Because the remarkable effects induced by colchicine treatment are of great economic value to plant breeders as well as to students of genetics and plant evolution, this treatment is now being studied by many workers in many places and its literature grows by leaps and bounds. It has furnished a long-desired key for altering the genetic characteristics of plants, and its employment promises to result in new horticultural varieties of fruits, vegetables and ornamentals.

Among other lines of investigation which have received attention in recent years at Cold Spring Harbor may be mentioned one that deals with the problem of the human inheritance of taste thresholds for such substances as phenylthiocarbamide and mannose. Dr. Blakeslee's demonstrations of his "taste tests" will be remembered by every one who visited the American Association's science exhibitions at New Orleans, in 1931, and at Richmond, in 1938.

Since 1935, President Blakeslee has been director of the Department of Genetics of the Carnegie Institution of Washington. He has received many scientific honors and has been active in many science societies. He holds membership in the National Academy and in the American Philosophical Society. He joined the American Association for the Advancement of Science in 1902 and was elected to fellowship in 1909. He was secretary of Section G (Botany) in 1916–17, vice-president for that section in 1917–18. And now he is president of the association for the year 1940.

Burton E. Livingston
Johns Hopkins University

AWARD OF THE AMERICAN ASSOCIATION PRIZE

Each year for seventeen successive years, the American Association for the Advancement of Science has awarded its Thousand Dollar Prize to a scientist for work on which he has presented a paper at its annual meeting. The prize at the meeting in Columbus in December was awarded to Dr. I. I. Rabi, of Columbia University. The work of Dr. Rabi throws important light on problems of the constitution and properties of subatomic units.

Any one who keeps even slightly in touch with science is at least vaguely aware of that remarkable body of doctrine known as "the quantum theory." It has achieved very striking successes, but at the price of converting the physicists' picture of the world into something which in parts is very like a surrealist fantasy. If one were asked to choose the most incredible of its features, one could hardly do better than take what is cumbrously called "space quantization." Yet it is this very extravagance of the quantum theory which is now the best attested!

Space quantization is a quality of the elementary particles of which the world is made up, in so far as these are magnets. All electrons and most nuclei (and for that matter, atoms in general) are magnets. They are magnets by reason of the fact that they comprise rotating

electrical charges; for the spinning or rotation of a charge-bearing body converts it into a magnet. Also by reason of the rotation or spinning, the electrons and the magnetic nuclei have angular momentum. The startling quality of which I have to speak is a quality of angular momentum really, but so closely linked is this last with the magnetism, that it will be excusable as well as convenient if from now on I call it a quality of the magnets.

To state it briefly, then, this fantastic quality is the following: these elementary magnets, when in a magnetic field, are constrained and obliged to incline themselves — "orient themselves," we say— in one or another of a very few permitted directions. The electron and the proton are obliged to choose between two "permitted orientations"; these bear the descriptive names of "the parallel orientation" and "the anti-parallel orientation." The deuteron enjoys three possibilities; those two and "the transverse orientation." There are more complicated cases, but we shall not consider them.

DR. I. I. RABI

Nearly twenty years ago the physicists Gerlach and Stern undertook to test the notion of space quantization, by projecting a jet of silver atoms horizontally through a vertical magnetic field. Had the field been uniform, the jet would have been unaffected, for each atomic magnet would have found its north pole pulled upward just as strongly as its south pole was being pulled downward, and the net force would have been nil. The field, however, was designed to increase very rapidly in strength in the upward direction, and therefore any atomic magnet traversing it found one of its poles subjected to a stronger field than the other pole, and consequently suffered a net force upward or downward. In this non-uniform field the jet of silver atoms was divided into two, one consisting of atomic magnets with their north poles pointed straight up the field, the other of magnets with their north poles pointed straight down the field. There were no intermediate cases, and the most incredible assertion of quantum theory thus became an undoubtable fact! (One of the experimenters has since admitted that when he planned the experiment he expected it to confute the theory, not to substantiate it.) This particular result refers to the electron, since the silver atoms play here the part of inert carriers of a single electron which alone is oriented.

This is the method of experiment which Rabi and his school have refined by an amount comparable with the progress in the making of spectroscopes from Fraunhofer's grating of wires to the ruled gratings of the twentieth century. With the method thus refined they have made a remarkable variety of striking experiments and significant measurements. But to introduce the result reported by Rabi at Columbus, I must now remove the implication that as one of these magnets is passing through a field, its orientation is irrevocably fixed. So it was, in all the experiments up to 1937; but Rabi conceived how to make the magnets change their orientations midway in their flight. Now in changing their orientations, they either lose or acquire energy, since it requires work to alter the inclination of

a magnet with respect to a magnetic field. If the change of inclination is in the sense corresponding to loss of energy, this lost energy is radiated in the form of a photon or corpuscle of light. The loss of energy is so very small that the light in question often belongs to the range of wave-lengths which ordinarily we do not consider as being light at all, but rather as radio waves. What Rabi therefore conceived was a method for producing radiations from atoms and molecules, which often are of radio frequencies.

The method serves not only for producing these rays, but for detecting the "transitions"—*i.e.*, the changes in orientation—which produce them, and for ascertaining their wave-lengths, the magnetic moments of the magnets and a host of other details of the molecular structure. As the jet pursues its course through the magnetic field, it passes through a region where an oscillating radio-frequency field can operate on it. At certain critical frequencies—or let me say critical wave-lengths—the transitions occur. The molecules which undergo them follow altered courses through the remainder of the magnetic field, and therefore miss the detector, which is so placed as to catch only those which are unaffected. At the critical wave-lengths the detector thus reports a falling-off in the strength of the jet. These are the wave-lengths of the rays which the particles emit or absorb. When they are measured, the magnetic moments of the reoriented particles can be calculated.

Now I must say what these particles are. The experiments were performed on hydrogen molecules, each containing as its pair of nuclei either a pair of protons or a pair of deuterons or one of each. The re-orientations which are discerned are those of the protons and deuterons, sometimes turning singly and sometimes turning as though welded into a single rigid whole. Here the molecules are serving as carriers for orientable nuclei, as in the Gerlach-Stern experiment the silver atoms served as carriers for orientable electrons (and, by the way, Rabi at Columbus described observations on the re-orientation of such electrons). The magnetic moments of the proton and the deuteron are computed with accuracy hitherto undreamed of, while finer details of the phenomena reveal the interactions between the two nuclei of the molecule, as also those of the nuclei with the revolving electrons which complete the molecule. For those to whom the terms "spectroscope" and "Zeeman effect" are familiar, I may say that the molecular jet passing through a system of cleverly designed magnetic fields now becomes a spectroscope of resolution far surpassing the best optical device and is applied in these experiments to a Zeeman effect of extraordinary smallness and subtlety.

KARL K. DARROW
BELL TELEPHONE LABORATORIES

SYMPOSIUM ON BLOOD, HEART AND CIRCULATION

TO-DAY heart disease stands first in the list of the causes of death. Therefore, it was most timely for the Section on Medical Sciences of the American Association for the Advancement of Science to initiate and organize as its contribution to the annual meeting at Columbus, Ohio, a symposium on the blood-vascular system. The layman is quite callous and complacent to the oft-repeated accounts of the achievements of modern medicine in preventing and controlling such age-old plagues as yellow fever and smallpox, maladies which the majority of our younger clinicians have never seen, and now, confronted with mortality statistics,

raises the question as to what is being done about heart disease.

During the four decades or more that have passed, laboratory and clinical investigation has increased by leaps and bounds, and in this forward march of progress cardiovascular research has been in the front rank. One may consider as an example the field of electrocardiography. Einthoven, a physiologist of Leyden, not only designed the string galvanometer, but employed the electrocardiograms obtained by the use of this instrument in the diagnosis of simple arrhythmias, hypertrophy of cardiac chambers and changes in position of the heart. Sir Thomas Lewis and his pupils extended the work to include curious ventricular beats and many obscure rhythms and sequences of the heart. Since these epoch-making experiments, the pendulum has swung across the Atlantic, and to-day leadership in the study of the heart rests in our midst. As a matter of fact, American eminence stands out conspicuously, and many of the foremost contributors in their fields participated in the program at Columbus.

Forty papers in the nature of reviews were prepared for the symposium. Eight dealt with various aspects of the composition of the blood. In one, attention was directed to the recent advance made in controlling hemorrhage as a result of the discovery and isolation of vitamin K. This substance is essential to the production of prothrombin, which plays a definite rôle in the clotting of blood. The administration of vitamin K has important implications in the field of human therapeutics, particularly with those individuals born with the tendency to bleed (hemophiliacs). A second paper considered the mechanism of antibody formation in susceptible hosts. For nearly forty years physicians have been injecting suspensions of dead bacteria and their products into man and animals in order to stimulate them to provide their own defensive mechanism against diseases such as typhoid fever and diphtheria. The efficacy of the procedure is manifested by the present low incidence of these scourges of former years. Coincident with the development of immunity, protective antibodies appear in the blood; it is their presence that forms the basis for borrowing immunity for therapeutic purposes from specially treated horses or in certain instances the serum of convalescents. The actual site of antibody formation has been an enigma, but Dr. Florence Sabin with the aid of a highly colored synthetic chemical compound feels she has found the answer to the problem. The foreign material, following its introduction into the body, is carried to or attacked *in situ* by cells of the reticulo-endothelial system. These cells, which are concentrated in such places as the lining of the liver sinuses, in the spleen and bone marrow, then dissolve the foreign agent and in turn synthesize and liberate into the blood stream from their cytoplasm immune globulins, the protective agents.

Of all the organs in the body, the heart has had the most attention by clinicians, for, when the heart stops, the patient is pronounced dead. The first of the papers devoted to this organ was a detailed account of the main coronary arteries, those vessels which supply the blood to the heart muscle itself. The coronary artery pattern is not the same in any two human hearts and may in general be placed in three groups depending on what coronary artery predominates in the blood supply to the heart. Hearts which are supplied mainly by the left coronary artery are most susceptible to coronary arteriosclerosis, while those in which both the right and left arteries function properly are least susceptible to this disease. Many factors influence the flow of blood

through the coronary vessels, although it would seem that the purely mechanical ones are the most important.

The possibility of the production of cardiac lesions by non-penetrating wounds to the chest, particularly in connection with modern automobile accidents, is giving the clinician considerable concern. It appears from the experimental evidence that the apprehension is warranted, since slight hemorrhage and contusion of the various parts of the heart are actually associated with non-penetrating injuries. The possibility of fatal arrhythmias developing following external chest injury without pathological evidence of cardiac injury was demonstrated in dogs and postulated in man.

Shortness of breath, or dyspnea, is recognized by the laity as well as the medical profession to be a most common symptom of heart disease. Investigations point clearly to congestion of the lungs, a sequela of heart disease, as the most important cause of dyspnea.

The final papers on the symposium were devoted to the subject of hypertension, or high blood pressure. Physiologists and clinicians have linked the kidney with this condition for many years without an adequate understanding of the mechanism involved. The evidence suggests the association of persistent hypertension with a variety of renal injuries, the ultimate cause being humoral rather than nervous in origin. The old belief in "renin," a renal extract, as being responsible for the development of hypertension is now questioned, and in its place there is the possibility that the absence of a humoral agent from the diseased kidney may play a part in the production of this condition.

The symposium was an outstanding success. It afforded an opportunity for both clinicians and laboratory workers to meet and discuss their common problems. The clinician confronted in his practice with unusual conditions must find their solution in laboratory experimentation. In the past the practitioner has not used, particularly in connection with heart disease, the findings of the laboratory workers, and it is only when opportunities for free discussion are provided that advance will be made in the conquest of the leader of the causes of death.

MALCOLM H. SOULE
UNIVERSITY OF MICHIGAN

THE SCIENTIFIC EXHIBITION IN COLUMBUS

AT its annual meeting each year, the American Association for the Advancement of Science has a scientific exhibition participated in both by scientists showing their latest researches and by publishers of scientific books and the manufacturers of aids to scientific research. The science exhibition at the Columbus meeting of the association was a fine success. Dr. Farr, of the Boyce-Thompson Institute for Plant Research, who presented one of the most interesting research exhibits, writes as follows:

> May I take this occasion to express to you our appreciation of the facilities offered to our department in connection with the recent Annual Exhibition of the American Association for the Advancement of Science. The effort which had been expended in preparing the material for our exhibit was more than fully repaid. I know of no other plan in effect which succeeds so well in fostering constructive and uninterrupted discussion of research data in the presence of research material than that offered through the medium of the Annual Exhibition.

And a scientific equipment company, which has been an exhibitor at meetings of the association for more than ten years, says:

> May we say a word of appreciation at this time for the very noticeable improvement in the atmosphere in the convention hall. . . . The result was more natural and there were more friendly conversations, and more opportunity for the professors to ask questions and for us

A VIEW OF THE SCIENCE EXHIBITION
FROM THE BALCONY OF THE AUDITORIUM, SHOWING THE SCIENCE LIBRARY IN THE BACKGROUND.

to answer them as completely as possible without interruptions.

The policy of the association, to combine research and scientific-commercial exhibits, seems to have met with approval. It was interesting to find a Nobel prize winner exhibiting side by side with a company which supplies men of science with the media for their work. The association appreciates the cooperation of those who participated in what is probably the most unique semi-commercial exhibition in America.[1]

It is to be regretted that the fine exhibits of some of the exhibitors specializing in the chemical field did not re-

[1] American Association for the Advancement of Science; Bakelite Corporation; Battelle Memorial Institute; Bausch and Lomb Optical Company; Biological Abstracts; The Blakiston Company; Boyce Thompson Institute for Plant Research; Carnegie Institution of Washington, Department of Genetics; Fred S. Carver; Central Scientific Company; Chicago Apparatus Company; Clay-Adams Company; The Coleman and Bell Company; Commercial Solvents Corporation; Denoyer-Geppert Company; Eastman Kodak Company; The Exact Weight Scale Company; Ford Motor Company; Gradwohl School for Laboratory Technique; Graf-Apsco Company; Guthrie Clinic, Robert Packer Hospital; Dr. Robt. T. Hance, Duquesne University; Institutum Divi Thomae; Dr. Chas. T. Knipp, University of Illinois; Leeds and Northrop Company; Dr. B. J. Luyet, St. Louis University; McGraw-Hill Book Company; The Macmillan Company; Merck and Company; National Geographic Society; The Ohio State University and Cooperating Institutions; Phonetics and Chemical Abstracts; Engineering Experiment Station; Industrial Research Foundation; Prentice-Hall, Inc.; W. B. Saunders Company; The Science Press Printing Company; Seoscope, Incorporated; Society for Research on Meteorites; Spencer Lens Company; Student Science Clubs; Vassar College, Department of Botany; U. S. Bureau of Plant Industry cooperating with the Biological Institute of Sao Paulo; U. S. Department of the Interior, Geological Survey; U. S. Housing Authority; University Presses; Virginia Agricultural Experiment Station cooperating with Duke University and North Carolina Department of Agriculture; W. M. Welch Scientific Company; John Wiley and Sons, Incorporated; Wilkens-Anderson Company; The Williams and Wilkins Company.

ceive all the attention that was desired. This was probably due to the inclement weather and the fact that the research exhibits in chemistry were held at the Ohio State University, at a distance of several miles from the general exhibition hall.

The Science Library, the purpose of which is to display scientific books published during the year, has become increasingly popular. It provides an unequalled opportunity to examine the year's books in all fields of science.

O. C.

THE WASHINGTON DINNER IN HONOR OF JULIAN S. HUXLEY

ONE of the important functions of the American Association for the Advancement of Science is to diffuse science among men. That is also one of the chief functions of the Smithsonian Institution and the primary function of THE SCIENTIFIC MONTHLY. Consequently the Association, the Smithsonian Institution and THE SCIENTIFIC MONTHLY arranged a large invitation dinner in honor of Dr. Huxley, which was held in Washington on the evening of December 2. At this dinner Dr. Huxley spoke brilliantly on "Science, Natural and Social." Of course, only a brilliant address could be expected from a man of such distinguished family and of such varied and exceptional accomplishments. He is a grandson of the great T. H. Huxley, a contemporary of Charles Darwin and an unanswerable advocate of biological evolution, a man whose public addresses were so brilliant that they drew distinguished writers and clergymen and

DR. JULIAN HUXLEY
PHOTOGRAPHED IN THE OFFICE OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, SHORTLY AFTER HIS ARRIVAL IN AMERICA.

AT THE DINNER IN HONOR OF DR. HUXLEY
DR. JULIAN HUXLEY, DR. F. R. MOULTON PRESENTING DR. BOWMAN TO THE AUDIENCE, AND DR. ISAIAH BOWMAN WHO INTRODUCED THE GUEST OF HONOR. THE LADIES AT THE EXTREME LEFT AND RIGHT ARE: MRS. BOWMAN AND MRS. MOULTON.

lawyers for the pleasure and advantages they obtained from listening to a great master of style. Dr. Huxley is a grand-nephew of Matthew Arnold, critic and poet; nephew of Mrs. Humphry Ward, novelist; son of Leonard Huxley, biographer and historian; and half-brother of Aldous Huxley, essayist and novelist.

One feature of the dinner made it a memorable occasion. Dr. Isaiah Bowman, the first lecturer to Great Britain under the arrangement between the American Association and the British Association, introduced Dr. Huxley, who at the time was scheduled to deliver, three weeks later at Columbus, the first British lecture before the American Association. Thus fortune and forethought brought together at a dinner in their honor in Washington the first two exchange lecturers of two of the greatest scientific organizations in the world, one British and one American. It is hoped that on similar occasions in the future scientists of many countries will join in considering the problems of civilization.